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REMARKS

ON

MR. EULER'S Treatise of *Motion*,
Dr. SMITH's compleat System of *Opticks*,

AND

Dr. JURIN's Essay upon Distinct and
Indistinct *Vision*.

By BENJAMIN ROBINS, F. R. S.

Hæc eo animo accipi velim, quo ego accipiam, quoties acciderit, ut aliquis mihi errores meos indicet. — Boni autem viri munus esse puto, non aliorum peccata dissimulare; sed potius omnes homines, si fieri posset, ab insitiæ tenebris in lucem veritatis asserere.

Petr. Nonius de Errat. Orontii Finæi. In præfat.



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P R E F A C E.

IT has not been uncusomary amongst mathematicians, without any impeachment of their candour, by exprefs treatises to correct important mistakes of their cotemporaries. Thus the errors of the Cardinal Cusanus were shewn by Regiomontanus, those of Orontius Finæus by Petrus Nonius, of Joseph Scaliger by Vieta and Clavius, of Longomontanus by Dr. Pell, of Gregory St. Vincent by Huygens, of Hobbes and Du Laurens by Dr. Wallis, and of Sinclair by James Gregory. And I shall here take leave after these examples to make some remarks upon two treatises, which have lately come abroad; Mr. Euler's discourse entitled *Mechanica sive motus Scientia analytice exposita*; and the Compleat System of Opticks by Dr. Smith.

FROM the time that Des Cartes advanced the calculations of algebra into the place of geometrical demonstrations, not only the works of the ancients, but at length the greatest part of mathematical writings have been neglected; and at present the mathematical sciences are generally considered as attainable by as little study as the operations of arithmetick. So much of the elements as can be learnt from some crude collection out of Euclide, with a few of the primary properties of the conic sections joined to some exercise in algebraical computations being thought at present, a stock sufficient to constitute a considerable geometer; and if to this be added a knowledge of the method of calculating used, where the doctrine of fluxions is required, without any true apprehension of the principles of that doctrine, the artist is supposed to have penetrated into the utmost depths of this science. Though by

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this means not only all elegance and taste are lost, but many direct errors have been committed by persons, compleatly adorned with these modern accomplishments, in their writings on a subject, of which they had not read enough to render it familiar to them, or their conceptions concerning it clear and distinct. Whoever could in the least contribute to put a check to this inundation of barbarism, would certainly perform no inconsiderable service to a branch of learning, once esteemed the first of the sciences, but at present reduced to a kind of mechanical practice, wherein the reasoning faculties are little concerned.

*IN the first of the treatises I design to examine, the author has unfortunately followed the principles of his calculus with so little caution, as even to contradict Euclide himself (a); and confides in them so entirely as to persuade himself, against the voice of common reason, that a body may be drawn to a center by centripetal forces, where though the velocity is perpetually increasing, yet at the center the body shall at once by some unknown cause be entirely stopt (b), or, which is more strange, annihilatd (c), and in other cases be turned directly back again (d); that a resisting medium may be supposed endued with no other power, than that of retarding the body's motion, yet shall cause a body projected directly upward to deflect into a curve line (e). We find the author of the second proposing as discoveries, what has been the received opinion of many ages (f); reviving maxims so deservedly exploded, that no man, who reflects, can hesitate about their falshood, whenever his eyes are open (g); deterred from attempting
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(a) pag. 8. 23.

(b) pag. 11.

(c) pag. 23.

(d) pag. 11. 21.

(e) pag. 28.

(f) pag. 56.

(g) pag. 42.

P R E F A C E.

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the solution of a cubic equation (a); exhibiting inconclusive demonstrations (b), and even unapprized of the meaning of so familiar a term as the similarity of figures (c).;

BUT how far these discourses are examples of the preceeding reflection, I shall not presume to determine; nor yet what claim the writer on distinct and indistinct vision may have to mathematical knowledge of any kind, since he has not given any publick specimen of great pretensions that way. Independent of these considerations as the first is intended for a very general explanation of motion, and the other promises no less than a compleat system of so extensive and interesting a subject as opticks, I hope these animadversions upon the failings, which I imagine myself to have found in them, will not be disapproved of. Since such general writings are usually had recourse to by those, who are first inquiring into any subject; it is not an unuseful labour to obviate the misinformation, to which learners by the errors in works of this nature must be expos'd.

I shall also add some Remarks on the Essay upon distinct and indistinct vision subjoined to the compleat system of opticks, that the profuse praises bestowed on it by the editor may not occasion any one to be misled by the explanation there attempted of some of the principal points in Sir Isaac Newton's doctrine of light and colours, in which that great man's sense appears wholly unknown to this writer.

I have chose to deliver my self every where with great plainness, and without those apologies and excuses, with which writings of this kind do so often abound; as such affected complements upon these occasions have always given me offence in others. In

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(a) pag. 55.

(c) pag. 70.

(b) 67, 72, 75, 77.

relation to Mr. Euler, who is no otherwise known to me, than from his works, though I have explained my thoughts without reserve; yet I hope free from any undue severity. In what concerns the other two gentlemen I have also made it my endeavour to avoid all excessive exaggerations of the faults, I have censured. But if any exceptionable expressions may have ever chanced to escape me, I trust, I shall easily be excused by them, who very well know the uncommon liberty, which has of late been taken with myself. To be more explicit, as the writer on distinct and indistinct vision is the reputed author of the late dissertations under the name of Philaethes Cantabrigiensis, and the other gentleman is not only suspected of being his associate, but merited, no doubt, by singular services that high strain of complement, with which Mr. Faber is in one of those papers addressed; I hold my self under no further restraint, where I have been used with such gross ill manners, than what a just deference for truth requires. I am thus free, not only because I think the treatment, I have received without the least provocation on my part, calls for some resentment; but also that I may do these gentlemen the justice to give them an opportunity of acquitting themselves, if they are falsely accused.

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Pag. 12. l. 24. &c. leg. this determination of the velocity beyond the center comes out impossible.

REMARKS
ON
Mr. LEONARD EULER'S
TREATISE
ENTITLED
MECHANICA.

THE author of the first of the two books, I here intend to examine, was a scholar of the famous Mr. John Bernoulli, and has formerly published several pieces, which have met with such applause, that he has been invited to the new Academy of Sciences erected at Petersburg, where he now enjoys the honourable title of Professor of the Sublime Mathematics. As this performance is composed on a very noble subject, which has so justly acquired to our great Sir Isaac Newton an unrivalled reputation; its merit has been celebrated in the various literary Journals of different countries with high encomiums.

THIS treatise after stating the general properties and affections of motion considers at large the effects both of impulsive forces, and of resisting mediums on moving bodies.

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THE first proposition is, “ That every body, “ which either by an absolute or relative motion “ is transferred into another place, must pass “ through all the intermediate places.” This proposition is, I think, merely identical; the idea of passing through these intermediate spaces being necessarily and obviously included in the idea of motion.

THE third proposition, “ That in any unequal “ motion the least elements of the space described “ may be conceived to be passed over with an “ uniform motion,” is not universally true.

IN many cases indeed the deductions founded on this position will be just, if we consider it as a figurative expression, meaning no more, than that the ultimate proportion of two contiguous spaces described in equal times by any variable motion is the proportion of equality. But wherever this interpretation cannot be applied, there this proposition will not only be faulty in its method of conception, but all the conclusions founded on it will also be erroneous. This will most remarkably be the case, when those spaces are compared together, which a body accelerated by any force describes in the beginning of its motion; for the ultimate proportion of the first of two contiguous spaces thus described in equal times to the second is not that of equality, but the ratio of 1 to 3, as is well known to every one acquainted with the common theory of falling bodies. This proposition will also fail, when applied to the estimating the increments of velocity produced in any moving body by any accelerating force; for if the velocity receives by the action of an uniform force equal additions

ditions in two equal contiguous portions of time, the additions to the spaces described in consequence of these increases of velocity will be as 1 to 3, instead of being to each other in the ratio of equality, as they ought to be, if this proposition were true.

MR. EULER in the annexed scholium has some obscure suspicions of the fallacy of this proposition: but as he applies it without scruple through the remaining part of his book, even in those instances, in which he here seems doubtful; it is evident, that the genuine principles, from whence alone all deductions of this kind receive their accuracy and rigour, were unknown to him; especially since some of his most capital errors arise from an unwarranted adherence to this proposition, as we shall more distinctly shew hereafter.

THE 7th, 8th, and 9th propositions relating to the continuation of a body in its state either of rest, or of equable motion in a right line, unless disturbed by some external force, ought to be proposed, as facts only; it being from experiments, and not from any metaphysical notions about bodies and motion, that the truth of these principles can be evinced. No wonder therefore, if Mr. Euler's pretended demonstrations of these propositions be altogether inconclusive and inconsistent.

IN the 11th proposition he is so little apprized of the relation, which a demonstration ought to have to a proposition, that here, though the proposition be general, the demonstration regards one particular case only, and the general proof is postponed to a scholium, in which he has so forgot his proposition, that he thinks it necessary to prove in form, that the ratio of a to b is given, which is

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the very thing, the proposition supposes. Nay more, in order to make this proof he has assumed a triangle to be given in species, merely because one angle is given. In order to prove that NL or BM will have to AL (fig. 9. Tab. I.) a given proportion, it is asserted, that the triangle ALN is given in species; but this is not true, unless this proportion of BM to AL is first supposed to be given, nothing being determined in that triangle independent of that supposition, but the angle ALN.

MR. EULER'S 14th proposition, very indistinctly proposed, contains in the solution there given, and the corollaries annexed no less than four capital errors.

FIRST it is said at the end of the solution, that the right line AD (fig. 1. Tab. II.) may be taken for the path described by the body, when acted on by a power in the direction AC. But this is false even on the confused principles of indivisibles. For were it true, the effect of the power in inflecting the body from its rectilinear motion, if estimated between D and A, would not be in the duplicate proportion of the times of its action, as it ought, but in the simple proportion of those times. The truth is, that every part of the motion however minute must be considered as a curve touching AB, and not as a right line intersecting it.

SECONDLY we are told in coroll. 2, that dc is equal to $\frac{Db}{dt}$, that is, to speak sense we are to conceive, that in the same time, in which the body with the velocity, it has at A, would describe the
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the space AB, or the equal space Ab, it would with the additional velocity, it is augmented by at D, describe the space Db. But this is not true; for this increase of velocity is not acquired immediately in the point A, but during the whole time of the motion from A to B; and therefore, the ultimate relations being considered, Db is but half the space, that would be described during the passage from A to D by the increase of velocity at D, that is in the infinitesimal jargon, dc is not equal to $\frac{Db}{dt}$, but to $\frac{2Db}{dt}$. Mr. Euler has been led into this error by relying upon his 3d proposition quoted by him in the preceeding corollary, which we have above observed to be a false one.

THIRDLY in the 4th corollary the same error occurs again, but in another form, being compounded with the cosine of the angle BAC.

FOURTHLY in the 5th corollary the angle of inflection of the direction of the body at D from the direction AB is determined to be the angle BAD, that is, the path described by the body is supposed to be the right line DA; but this is not true, as we have shewn above. Considering then the path described to be a curve touching AB in A and passing through the point D, as it ought, we must measure the inflection by the angle, that the tangent to this curve in the point D makes with the tangent AB, and this angle, ultimately considered, will be double BAD, that is, its sine in Mr. Euler's phrase is not $\frac{Bb}{BA}$, but $\frac{2Bb}{BA}$.

THESE errors now mentioned are variously compounded with each other in the 6th, 7th, 8th,
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and 9th corollaries ; so that not only the general solution of this problem, but 7 of the annexed corollaries are altogether erroneous, and this even on Mr. Euler's own principles.

THE solution of the next proposition (prop. 15.) deserves particular notice. Here AB represents the space, which a body with the motion, it has in A , would describe in the direction AE (fig. 2. Tab. II.) during any small interval of time, and Bb the space, which the same body would have described from rest in the same interval by the power acting in the same direction AE ; that is, Ab represents the whole space described in some small interval by the uniform velocity of the body in A , and by the action of a power in the same direction. Now we are told, that in another equal interval by the velocity acquired in b uniformly continued it would describe another space equal to Ab . But this is false. For since the velocity of the body is perpetually changing from A to b , the space Ab cannot be conceived, as described by the whole velocity acquired at b , but by some velocity intermediate betwixt that and the velocity at A ; the space then, that would be described by the velocity at b in the next equal interval, will be greater than Ab , and if the action of the power was uniform from A to b , (as Mr. Euler by styling the interval of time an infinitely little one supposes) it will be equal to $Ab + Bb$; instead therefore of the equations in Mr. Euler $Ab = AB + ao$, $bc = AB + 2ao$, $cd = AB + 3ao$, $de = AB + 4ao$, there ought to be substituted $Ab = AB + ao$, $bc = AB + 3ao$, $cd = AB + 5ao$, $de = AB + 7ao$. All this has been so often shewn by every writer on the common theory of falling bodies, that I am surprized at Mr. Euler's mistaking so plain a matter. But these are
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not all the errors of this proposition. For Mr. Euler having by the application of his 3d proposition concluded, that the celerity of the body A is increased in the same proportion, as the increments of the spaces described by it, we may shew from the equations, we have rectified above, that taking this principle for granted, the increments of the celerity can never be proportional to the times, in which they are produced ; for if the times are taken in the proportion of the numbers 1, 2, 3, 4, 5, &c. the increments of the spaces, and consequently of the velocities, if Mr. Euler is to be believed, will be in the proportion of 1, 3, 5, 7, 9, &c. a proportion not only different from that of the times, but, if the extreams are compared, perpetually receding from it. Another principal error then of this proposition is, that notwithstanding these erroneous suppositions the conclusion, “ that the increments of the celerities are ultimately as the times, they are produced in,” is indisputable, as has been frequently shewn by other writers. Now it can be only by a complication of errors, that true deductions are ever derived from false principals.

THE process annexed to the 18th proposition is absolutely inconclusive, and unworthy the name of a solution.

AS Mr. Euler tells us in the 2d Corollary of the 19th proposition, that he has been particularly solicitous about the rigid demonstrations of these last propositions ; I have no design to charge this author with haste or negligence on account of these errors ; but I consider them solely, as the effect of that inaccuracy in conception, to which the differential calculus is disposed to betray its admirers.

IN the solution of the 21st proposition and its 1st corollary all the errors of the 14th are continued; and consequently all the equations there given are on that account false. But in the 2^d corollary Mr. Euler has fallen into an error of still a stranger nature, even following the principles of his differential calculus to an express contradiction of a proposition of Euclide's elements, which shews, that the angle at the circumference of the circle subtended by the line AD (fig. 6. Tab. II.) not the angle at the center, is equal to the angle BAD; and consequently it is not the triangle AOD, OD being supposed to be drawn, but the triangle formed on AD by lines drawn from that point, where AO continued meets the circumference beyond the center, that is similar to the triangle BAb; and therefore Bb is not to AB as AD to AO, but as AD to 2AO. It is not then the radius AO of the circle touching the line AB and passing through the point D, but the diameter of that circle, which Mr. Euler has here assigned, that is, $2AO$ is $= \frac{AB \times AD}{Bb}$, not, as he makes it, $AO = \frac{AB \times AD}{Bb}$.

THIS determination of the radius of curvature in the present corollary is, as it were, the basis, on which the future examination of all curvilinear motion does immediately depend; and were not this error compensated by those, we have taken notice of already, and others, that we shall hereafter remark, there could not be one proposition in all this volume, where the body is supposed to move in a curvilinear track, but what would be erroneous. This proposition is indeed of such necessary use in the methods of proceeding followed in these volumes,

lumes, that it occurs perpetually ; but its falshood no where more eminently appears, than in the demonstration of the 1st proposition of the second volume, where it is more obvious, and more express, if possible, than in the present instance.

THE value of r then assigned in the 2d corollary, and used in the succeeding ones of this 21st proposition, is double, what it ought to be.

BUT in the 10th corollary this erroneous value of r is compounded with a value of dc , which we have shewn above to be but one half of what it ought, and by this means the equation resulting in this corollary is rectified ; the two errors ballancing each other.

THE sense of the 22d and 23d propositions may be briefly exhibited thus.

LET a, b, c, d , be parts of the body O considered as a Point, and OA, OB, OC, OD , (fig. 8. Tab. II.) a like number of forces applied to that Point, and let OG be the force compounded of all these. Then, if the parts a, b, c, d are supposed to be separately accelerated by the respective forces OA, OB, OC, OD , I say, that the motion of the common center of gravity of these parts a, b, c, d , will be in the direction OG , and with the same acceleration, as would be that of the entire body O , if accelerated by the force OG .

THIS is the true and genuine form, under which, I think, this proposition should have been expressed: the principle of a restoring force, which Mr. Euler has introduced, serving only to

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perplex the reader's imagination, and render the proposition unintelligible.

THE demonstration is equally inaccurate ; for in the first place it is no demonstration of Mr. Euler's proposition (of which indeed no demonstration can be made, as it contains no assertion, that is the proper object of demonstration,) and it is in the next place excessively artless and tedious, containing above 20 equations, when the whole may be easily dispatched by the application of a well-known proposition in statics, without any computation.

IN the beginning of the third chapter, which treats of right-lined motion, Mr. Euler has given Galileo's theory of falling bodies, in its own nature no difficult subject ; but it is here so compounded with differential computations, that this subject may be much better learned from what has been writ in a more simple manner by others.

IN the remaining part of this chapter he treats in general of the direct ascent or descent of bodies accelerated by any forces referred to a given point. And in his 1st scholium to his 32d proposition he is much perplexed how to determine the motion of the body, when it has passed the point C (Tab. II. fig. 13.) He apprehends no more necessary than making y negative in the expression of the solution given in the proposition ; and thence concludes, that if the quantity resulting is affirmative, the body will really pass the center C, but if it be negative, it is an indication, that the body will never pass the point C. He however owns, that in some cases this method of prosecuting the motion includes a contradiction.

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BUT though he has concluded, that in certain instances this proceeding will mislead us, because the result in those cases is different from what, he knows from other principles, it ought to be; yet instead of making the true use of this discovery, and suspecting the whole process, he still supposes the solution to be right in those cases, where he has not the same means of comparing the result with known principles.

NOTWITHSTANDING he discovers by the absurdity of the conclusion, that the method of solution, he has given, will in numberless instances fail in determining the motion of the body, when it has passed the center, he nevertheless in other instances, where the same absurdity occurs, supposes the motion of the body to be rightly exhibited by these fallacious operations; having advanced in the 1st and 2d scholium, and 5th and 6th corollaries the three following wonderful positions.

1st, THAT in many cases neither the motion nor direction of the body after it arrives in C, can be determined.

2d, THAT in other cases the body, after it arrives in C, can pass no further.

3d, THAT in another case there mentioned the body, when it arrives in C, will instead of proceeding forwards fly back again in the same direction, in which it fell.

IT is true, our author speaks of all these determinations with some diffidence and uncertainty, and seems to think them very mysterious, but he satisfies himself with a blind submission to his

computation, "*quicquid autem fit hic calculo potius quam nostro iudicio est fidendum.*" Nay in the last conclusion he seems even to have satisfied his judgment; for in the passage, he refers to in the following part of his book, he has copied Sir Isaac Newton, as he conceives, and not understanding his author seems to suppose himself sheltered under his authority.

Now the whole mystery, which has thus embarrassed our author is this, When y , the distance of the body from the center, is made negative, the terms of the problem are sometimes changed with it. The centripetal force being as some power of the distance expressed by y^n , where n may be any number affirmative, or negative, whole number or fraction; if, when y is supposed negative, y^n be still affirmative, the solution gives the velocity of the body in its subsequent ascent from the center; but if y^n by this supposition becomes also negative, the solution exhibits the velocity, after the body has passed the center, upon condition, that the centripetal force become centrifugal; and when on this supposition y^n becomes impossible, the determination of the velocity beyond the center is impossible, the condition being so.

IN the corollaries and scholium annexed to the 48th proposition, which considers the ascent of bodies from a center, he meets with the same difficulties, and is at great pains to account, how the solution, he has there given, should fail him in certain cases; and to satisfy himself he recurs to infinitely small, infinitely great, and other the like ambiguous phrases, the usual modern palliatives of error. But so little does he seem convinc'd

vinc'd with his own reasoning on this head, that in the 96th section of his second volume, he has in a similar case supposed the computation to be accurate; disregarding those objections, he here makes use of to prove it fallacious.

THAT the computation in this proposition is unskilfully applied, cannot be doubted of at first sight; since, when n is any affirmative number, the force in C absolutely vanishes; and consequently the body will in these cases remain for ever at rest in the point C, if once placed there. Whatever computation therefore pretends under these circumstances to exhibit both the time of passing from the very center through any given space, and the velocity by that means acquired, must undoubtedly contain under it some fallacy. The truth is, the time determined by this method is the limit only of that time, in which the same given space would be described, supposing the body not to begin its motion from the very point C, but to be placed at first any where short of it. Indeed whatever be the centripetal force, if it diffuse itself every way equally from the center, no body can be moved out of the center by it.

THE problem proposed by Mr. Euler in his 43d proposition is from the relation of the times of description to the spaces described to determine the law of the impulsive force.

HERE he is entangled in difficulties from not understanding, that the conditions of a problem often put a limit upon algebraical expressions. This Sir Isaac Newton has very well illustrated in his treatise of algebra by the following easy in-

instance. If it were required to inscribe any chord given in magnitude within a given semi-circle ; x denoting the versed sine of the arch, which will be subtended by the given chord, a the diameter of the circle, and b the chord, x will be $= \frac{bb}{a}$: therefore whatever be the given magnitudes of b and a , there may be always found a value of x , which will satisfy the algebraic equation : yet it is manifest that if b be greater than a , the value of x derived from the equation will be of no use for solving the problem, which in that case is impossible. In like manner Mr. Euler proposes a case of his problem, (in the scholium) where the time of the body's motion, shall be expressed by $\sqrt{2ax - xx}$, x denoting the space, through which the body has moved, and a some given length. Here he perceives that this expression cannot in all magnitudes of x express the time of the body's motion ; for when x is greater than a , the quantity $\sqrt{2ax - xx}$ diminishes, whereas the time already passed cannot be recalled.

BUT what he gathers from thence, is absolutely absurd ; that when the body is arrived at C, it must ever remain in that point, which he owns himself is past all conception ; nay more, that the velocity of the body, if it passed farther ought to be negative, which how to account for, he confesses himself unable. Indeed all that can be performed in this problem, is to find such an impulsive force, wherein the given expression shall for some space denote the time. And when this is discovered, the limits, to which this expression must be subject, will be known. Here it comes out, that the impulsive force will be reciprocally as the cube of the distance of the
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body from a point at a distance equal to a from the place, whence the body set out. After this is known, it is easy so to express the time, that the expression shall agree to the body's whole motion. The time of the body's passing between the center now found, and any other place, will always be denoted by $a - \sqrt{2ax - xx}$.

IN the following chapter Mr. Euler treats of right-lined motion in a resisting medium.

HERE in the scholium annexed to the 20th definition we are told, that all other laws of resistance except that, which observes the duplicate proportion of the velocity are merely imaginary, which is also repeated in the 1st corol. of prop. 50. This is so great a mistake, that no medium is yet known, in which the whole resistance is accurately in that proportion. Our author had not attended to what, Sir Isaac Newton expressly takes notice of, that in all fluids some share of their resistance, though much the least, is owing to some degree of tenacity between their parts, and that this resistance is the same in all degrees of velocity.

IN the 4th corollary of the 50th proposition our author tells us, that when $m > 1$, the body, before it arrived at A (Tab. IV. fig. 2.), had somewhere, as in C, an infinite velocity; the meaning of which strange assertion is no more, than that a body, which should set out from the point C, determined in the manner there laid down, with any velocity, however great, would lose all its motion short of the point A; and consequently could never reach that point.

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IN the third scholium of the same proposition Mr. Euler seems again surprized, that his computation should give an expression for the velocity, when in reality there can be no motion; but we have already explain'd this difficulty in our observations on the corollaries of his 40th proposition, and shall therefore say no more of it here.

AFTER the 53d, 54th, 55th, 56th and 59th propositions of Mr. Euler have been so excellently solv'd by Sir Isaac Newton, in the 8th and 9th propositions of the second book of his Principia, the weight of computation with which Mr. Euler has filled them, can, I think, be no otherwise excus'd, than by supposing him unskilled in Sir Isaac Newton's treatise of Quadratures, to which recourse ought always to be had, in the analysis of such problems.

IN the solution of the 61st proposition, we have this equation $dx = \frac{k^m dv}{gk^m - v^m}$, x representing the space pass'd through, and v a quantity in the duplicate proportion of the velocity acquired at the end of that space, k and g being known quantities; and we are told, that by the quadrature of curves (ope quadraturarum) v may be determin'd by x , however in the annexed scholium Mr. Euler dismisses this equation from any future examination, because its integral cannot be exhibited. But by the improvements the late Mr. Cotes has made to Sir Isaac Newton's doctrine of Quadratures, which were soon after their publication demonstrated by Dr. Pemberton, it is always possible by the circle and hyperbola to assign the relation of x and v , whether m be an integer or a frac.

fraction; and therefore a more ample prosecution of this equation might reasonably have been expected from a person so much in love with computation.

IN the first scholium of the 62d proposition we are told, that the velocity of the moving body always vanishes at C, because the resistance in that point is infinite. But this is false reasoning; since every infinite resistance will not instantaneously destroy a finite velocity. For example, should the density of the medium be reciprocally in the subduplicate proportion of the distance from C, and the resistance in the simple proportion of the velocity, which will be the case in this proposition, when m denotes the fraction $\frac{1}{2}$; notwithstanding such a resistance, the body may yet arrive at C, with any given degree of velocity however great.

IN the second scholium to the 66th proposition Mr. Euler observes, that the equation determining the quantity q in that proposition will not be changed, although instead of p or R any multiples of those quantities are taken. And as the magnitude of q in reality varies, when instead of R any multiple is taken; he accounts for this only by telling us in general, that a differential equation is more extensive than the integral, from whence it is deduced.

THIS vague and indetermined reason shews, why Mr. Euler considered this as a difficulty; which could scarce appear so to one duly apprized, that the fluxion of a logarithm of any quantity is the fluxion of that quantity, divided by the quantity; therefore whatever given number or

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quantity multiplies the quantity proposed, the same multiplies both the numerator and denominator of the fluxion, whereby the fluxion remains the same, however that number or quantity be varied; as it ought to do because the multiplying a flowing quantity by any given one only increaseth its logarithm by the logarithm of the given quantity.

IN the fifth chapter, which treats of curvilinear motion, every proposition from the beginning to the end does immediately or consequentially depend on those erroneous equations, which we have taken notice of in the remarks on the 14th and 21st propositions; and therefore, however true the conclusions here given may be; yet, as they are founded on wrong positions, they owe their coincidence with truth to chance only, by the accidental intervention of contrary errors.

BUT admitting the principles here made use of to have been truly and accurately obtained, yet there are other important exceptions both to the method of this chapter, and to particular conclusions contained in it.

IN relation to the method of investigation here made use of, it is necessary to observe, that the ancient geometers in their analysis, no less than in their composition, brought in aid, as occasion required, whatever had been discovered by others; and that investigation was by them the most approved, which contained in it no process, that could be supplied or contracted by any known proposition. On the contrary our algebraical gentlemen in the modern taste lay aside
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all regard to order, conciseness, or elegance in their conceptions ; and confine their whole labour to the single ambition of rendering all previous knowledge as little necessary, as possible, to their present conclusions.

MR. Euler has here so closely pursued this Scheme, as not to think of digesting even his own subject into any method, which might facilitate his analyses. Instead of premising these two general propositions, That of the equality of the areas described in equal times about the center of attraction, and That of the equality of the velocities at equal distances from that center both in the direct and curvilinear fall, whereby his following analyses might have been abbreviated ; he plunges without preparation into compound propositions, and contents himself with slightly deducing these, as corollaries, from a proposition [prop. 74.] to which they should have been premised.

Two such propositions, which are or ought to be the very basis of all investigations of curvilinear motion, certainly merited to have been particularly discussed in their full extent, instead of being thus derived, as it were, by chance, from a proposition not so extensive as they are. This is inverting the natural order of the subject, and necessarily involves it in obscurity and perplexity.

BUT to be more particular, the deductions in the 70th and 71st propositions needed not that pomp of differential equations and canons ; being easily deduced with more brevity without them, particularly the 3d and 4th corollaries of the 71st.

IN the 72d proposition, and its corollaries the effect of gravity compounded with a rectilinear motion is discussed with a degree of intricacy even surprizing in so simple a subject.

THE 73d, a very easy subject, is not much better treated.

IN the 74th proposition the radius of curvature is unnecessarily involved in the computation, to no other purpose than that of embarrassing the process, till it is exterminated.

THE solution of the 75th is unmeasurably prolix; for if from the center C a circle be described, whose radius is unity, u will in this circle be the cosine of the angle MCP, and the ultimate proportion of the increment of CM or y to the increment of u will be compounded of the ratios of MT to TC, CM to the radius 1, and the radius to the sine of the angle MCP, that is, of the ratios MT to TC and CM to $\sqrt{1-u^2}$; whence the differential equation $\frac{du}{dy} = \frac{1}{\sqrt{1-u^2}}$ is immediately deduced; to arrive at which Mr. Euler has employed near 20 equations, substitutions, restitutions, &c.

BESIDES the general obscurity, in which the 78th, 79th, 80th, and 81st propositions are here involved, there is in the 11th corollary of the 80th an error of so extraordinary a nature, as to merit a particular discussion.

IF the attraction directed to any center be reciprocally in the duplicate proportion of the distances from that center, and a body be projected from any point in a direction perpendicular to the line joining the center and that point with a certain degree of velocity, it is then confessed, that the curve described by this body will be an ellipsis, the center of attraction being its focus, and the point, from whence it set out, becoming that extremity of the axis, which is most removed from the focus. It is allowed too, that by diminishing the velocity of the projected body the species of this ellipsis is perpetually changing, approaching more and more to a right line, and that the time of half a revolution in this reduced ellipsis perpetually approaches to the time of the direct fall to the center. And since in these reduced ellipses the body, after it arrives at its lower apsis, will perpetually return back again to the point, from whence it was originally projected; since, I say, this will be true, however small the velocity is, with which the body is originally projected, that is, however near the species of the ellipsis approaches to a right line: Mr. Euler has falsely concluded, that in the direct fall, when both the original velocity, and the ellipsis vanish, the same return will take place, that is, that the body, the moment it arrives at the center, instead of proceeding forwards with the velocity it has there acquired, will immediately return back towards the point, whence it fell.

Now though this monstrous supposition obviously manifests its own absurdity, and carries its own confutation with it to every one, that at all considers it, yet that Mr. Euler may not be
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confounded in searching for those erroneous deductions which have here misled him, I will inform him that the reason, why, when there was any transverse velocity originally impressed, however small, the body constantly returned, after it was got at some small distance below the center of attraction, is this, that then the attraction to the center never conspiring in direction with the motion of the body, the direction of that motion by this means perpetually changes, till below the center, where it acquires its greatest velocity, it becomes perpendicular to a line drawn to the center, and consequently returns from that point with a motion altogether similar to that, with which before it descended. But if there is no such original velocity, and consequently no obliquity, this reasoning no longer holds good; for the body will now be ever found in the right line passing through the center; and therefore, when it has once acquired a progressive motion in this line, it must continue it, till having passed the center its velocity is abolished by the contrary action of the centripetal force.

IF the reader, after the numerous mistakes which this author has been guilty of, in relation to the perpendicular descent of bodies, is desirous of seeing them all in epitome, he need only consult the 2d scholium of the 91st proposition, which will fully satisfy his curiosity, and afford him an ample field for astonishment. He will here find our author's former discoveries even improved; that bodies drawn to a center by centripetal forces, which increase perpetually their velocity, shall not only on a sudden be stopt at the center in the midst of their swiftest

swiftest motion; but be moreover annihilated (a).

IN the demonstration of the 98th proposition we are told, that the radius of the curvature (Tab. IX. fig. 4.) at the point m , is $\frac{mv^2}{vE}$. But this is the old mistake of taking the radius instead of the diameter. For, since the line mn is a tangent in the point m , and V a supposed point of the curve, $\frac{mv^2}{2v}$, and not $\frac{mv^2}{vE}$, is the radius of the curvature at that point, as we have formerly shewn in our remarks on the 2d corollary of the 21st proposition.

IN the 4th corollary to this proposition the second differences of z and y are falsely assigned, being there but of half their proper magnitude. For to authorize the conclusions in this corollary Mm ought to be the chord of the preceeding arch continued; but since it is supposed to be the line, in which the body would proceed forwards from m , if not acted on, it must of necessity be considered as a tangent to that point m .

THE remaining part of this chapter is excessively obscure, tedious and ten times more compounded, than it need to be, of which take the following examples.

THE 101st proposition requires none of those complicated equations, it so much abounds with. The true method of solution is to conceive the motion of the body to be compounded of two motions, one of them parallel to the axis, and the

(a) Corpus, statim ac in centrum pervenerit, ibi evanescet.

the other performed in a plane perpendicular to the axis ; then considering these separately, the motion parallel to the axis will be ever uniform, being no ways changed by the attractive force ; and the motion in the plane perpendicular to the axis will be reduced to the simple contemplation of the figure described by a body acted on by a known central force, and having its direction in the same plane with that force ; then by combining these two motions together the situation of the body may at any time be known. From this method of solution the whole is reduced to principles already determined, and the conclusions in the two examples annexed to this proposition are attained without any computation.

IN like manner the 102d proposition abounds with a complication of differential equations no ways necessary. For if from the point *A* in the figure of that proposition a line be drawn perpendicular to the plane *APQ* let us denominate this line the axis. Then the force in *AM* may be always divided into two others, one of them parallel to this axis, and the other at right angles to it. But the parallel force being in the same direction with the force *MQ*, the body by the conjunction of these two forces will approach the plane *APQ*, and from the known quantities of these forces, and the known velocity towards this plane, the motion of acceding to this plane will be determined by the third chapter, it being under these circumstances a rectilinear motion : and by the force perpendicular to the axis a curvilinear track will be described round that axis in a plane, and to which that axis is always perpendicular ; and as this curvilinear orbit may be determined by the consideration of the motion in

a plane only, it is evident, that by this method the whole motion of the body will be determined by the means of known propositions, without the assistance of new principles, or the perplexity of new computations.

OF the superiority of this method of solution we will give the following instance.

SUPPOSE a body at M to be attracted partly to the point A (Tab. II. fig. 1.) by a force directly as MA , the distance from that point, and partly to the plane APQ by a force directly as MQ , the perpendicular distance from that plane.

To determine this body's motion let the force AM be divided into two forces AQ , QM , one of them, as AQ , perpendicular to the axis, and the other QM parallel to it. The first of these will be directly as the distance from the axis, since AM is always as the force directed to the point A , and by this force there will be described in the plane, to which that axis is perpendicular, an ellipsis having the intersection of the axis with that plane for its center. The other force parallel to the axis will be as MQ , the distance of the body M from the plane APQ , but the original force, by which the body M was attracted to the plane APQ , was also as this distance MQ ; therefore the whole united force, by which the body M is urged towards that plane, is as MQ ; the motion then of acceding to that plane is the same, as that of the direct fall of a body towards a center, to which it is attracted in proportion to its distance from that center. Therefore the motion of the body in a plane perpendicular to the axis being known, and the motion of

acceding in lines parallel to the axis to the plane APQ, or receding from it being known, the whole motion of the body is thereby determined.

FROM the known proportions between the force on M acting parallel to AQ, by which the ellipsis is described, and the separate forces, that by their conjunction in the direction MQ urge the body towards the plane APQ, the proportion between the time of one revolution in the ellipsis, and the time of one oscillation in a direction perpendicular to the plane APQ is easily assigned; and from hence the change of the position of the nodes, or of those points, in which the body passes through this plane in each revolution, is determined, the time of the bodies passing from one node to its opposite being the time of one oscillation. If therefore a line be supposed drawn from the center of the ellipsis to the node, in which the body was last found, the next position of the node will be determined by a line drawn also from the center of the ellipsis, containing with the first drawn line an area, that shall be in such proportion to the whole ellipsis, as is the time of one oscillation to the time of one revolution in the ellipsis.

THIS instance is the same, with the example Mr. Euler makes use of at the end of his 102d proposition (only an error of the press in the first line must be rectified, where instead of AP there is MP,) it also includes his 103d proposition, which is the determination of the nodes in this case. In the solution of this example Mr. Euler has filled several pages with the most abstruse and complicated computations. There are equations

tions arising to the 7th, 8th and 9th power, and involving the third powers of first differences, and the second power of second differences; there are introduced logarithms both real and imaginary; nay the simple determination of the nodes cannot be dispatched without the relation of tangents and sines to their respective arches.

THE last chapter of this volume, which treats of curvilinear motion in a resisting medium, is equally exceptionable with any of the preceding ones in respect to its method, and the operose computations, with which it is unnecessarily filled. But as we have already distinctly examined other parts of the book with regard to these particulars, we shall not enter into a farther discussion of them in this place, but shall conclude our remarks upon this volume by obviating a most erroneous determination of the motion of a body in a resisting medium, in the scholium annexed to the 112th proposition.

IN this proposition Mr. Euler inquires, what law of resistance combined with the uniform force of gravity acting in the direction AC (fig. 6. Tab. XI.) will compel a body to move in a given semicircle, as BAMD.

THE determination is, that, if the resistance in every point, as M, is to the force of gravity, as three times the distance MQ to twice the radius AC, and this resistance is supposed to conspire with the motion of the body, when it moves upwards in the quadrant BA, but to be oppos'd to it, when it moves downwards in the quadrant AD; then the body by the combination of this resistance with the force of gravity, will describe the

femicircumference BAD; and from hence in the scholium he concludes, the resistances in the quadrant AD being equal and altogether similar in their directions to those in the quadrant AB, that the body, when it arrives at D, will by means of these resistances already established return back through the femicircumference DAB to B.

THIS appears even to Mr. Euler wonderful, that a body at rest in D, and acted on by a force perpendicular to the line DC, for in this direction does the resistance act at the point D, should by means of this force be impelled in a line different from that perpendicular: but he thinks, he has solved the difficulty by telling us, that the force acting in the direction of the femicircumference at the point B is not absolutely perpendicular to the diameter in that point, but has an infinitely little deflection from the perpendicular.

WHAT the angle measuring this infinitely little deflection is, Mr. Euler will never be able to explain; it being an unintelligible reason urged in support of a false fact. For when the body arrives at the point D, it cannot return in the circumference DMA, but by the intervention of some superadded force. What has here misled Mr. Euler, is his want of discerning, that the determination of the law of the resistance supposes the body, when its resistance is investigated, to move with a motion oblique to the action of gravity; it being only by means of this obliquity, that the comparison of the resistance with the force of gravity can in this case take place. And consequently the points B and D are excluded

cluded from all inferences formed on these principles ; no more being determined, than that by such resistances, as are here assigned, the body will move from any one point of the semicircle to any other, provided the point, from whence the motion is supposed to begin, is neither B nor D.



REMARKS

REMARKS

ON

Dr. *SMITH*'s

COMPLEAT

SYSTEM of OPTICKS.

I SHALL at present proceed no farther in my remarks on Mr. Euler's book ; as I apprehend, what has been said, will be sufficient to prevent an intelligent reader from being misled by any of the mistakes of the succeeding part. And, I think, it appears, that the most of this gentleman's errors are owing to so strong an attachment to the principles, he had imbibed under that inelegant computist, who was his instructor, that he was afraid to trust his own understanding even in cases, where the maxims, he had learnt, seemed to him contradictory to common sense. But now I am to enter upon a more arduous undertaking, to pass censure upon the labours of a gentleman, who from his being a relation of the late most excellent Mr. Cotes, not only possessed all his papers at his death, but during his life enjoyed so uninterrupted a conversation with that great man, as furnished him with an opportunity of every kind of information, he could at any time desire. Therefore as we gather from Mr. Professor's
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REMARKS *on* Dr. SMITH. 31

propofals, that he has been employed more than twenty years upon this work, and as he has informed us, that Mr. Cotes himfelf before his death had the fame fubject under confideration, it were reasonable to expect no fmall fhare of perfection in fuch a performance. I fhall therefore endeavour to proceed in my remarks with all the circumfpection which, thefe reflections may feem to require.

THIS treatife fets out (*a*) with a very un-fkilful representation of a capital propofition in Sir Ifaac Newton's Opticks (*b*), by omitting an effential part of it, which Sir Ifaac Newton had expreffed under the form of an exception. For this exception is fo extenfive, that the refractive power of water, the moft common of all refrangible fubftances except the air, is almoft half as much again in proportion to its density, as that power is in the air, in glafs, and other terreftrial bodies. This power in all the oils is more than twice as much as in glafs, and in one fubftance, the diamond, near three times as much. The repeating the propofition afterwards at art. 189. entire, is no excufe for omitting the exception in this place; fince the conclufion, which Sir Ifaac Newton makes from this propofition, depends abfolutely upon that exception.

THERE foon follows, at art. 17, another inftance of this author's imperfect knowledge of the true theory of the action between light and bodies, where it appears, that when a ray within a denfer medium falls upon the furface of it, he did not apprehend, that it would be reflected
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(*a*) Artic. 5.

(*b*) B. II. prop. 10.

till such times, as the sine of incidence bore a greater proportion to the radius than that, which measures the refraction; whereas the ray will be reflected, as soon as the sine of the incident angle comes to that proportion. Upon this mistake he proceeds afterwards in the mathematical part, art. 472, 487, and talks of rays emerging in the tangent of a circle.

THOUGH in this popular part of his treatise the author might be allowed to omit some circumstances, which in mathematical demonstrations could not be dispens'd with; yet he ought never to have expressed himself, as he has done at article 21, where he describes the rays, which in opticks may be considered as parallel, to be such, in which the difference of their distances at any two given places is insensible: whence it should follow, that the rays of the sun are not to be considered, as parallel, unless their distances at the orbit of mercury and at the earth were not sensibly different.

IF in the figure referred to in article 32, O mean the place of the eye, for which it is used in the fourth chapter, the assertion in this article is not true. For the rays, which from P enter the eye, will none of them proceed from p , as here determined.

IN article 43 the ray, which within any glass lens makes equal angles with each surface, is said to pass through the middle point of the glass, whereas in all concavo-convex glasses that ray is inclined to a point in the axis, which is always without the glass. This oversight is indeed attempted to be corrected in the mathematical
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part, Art. 228 ; but very imperfectly. It is only said, that in these glasses this point will be a little without the glass ; whereas in such there is no distance, which it may not exceed. For the thickness of the glass is to the difference between the semidiameters of the spheres, of which the two surfaces are portions, as this distance to the lesser semidiameter.

IN article 95th this author has endeavoured to give such a proof of the proportion, which he has assigned between the light, we receive from the moon, and that we receive from the sun, as he imagined, might be easily apprehended ; though he informs us, that he discovered that proportion by other means. It follows, as far as it is just, directly from the proposition for the same purpose, of that excellent geometer James Gregory, in his *Geom. Par. Univerf.* page 144, and the general proposition there mentioned for determining the proportion between the degrees of light received from any planet, and from the sun, is repeated in David Gregory's *Astronomy* (a), a book in every one's hand. When this author was comparing moon-light with day-light, had he described them both in a similar manner, as was reasonable to have been done, he would have seen, how little the description, he has given of day-light, is to the purpose ; but it likewise appears from the result. For he first informs us, that the proportion, he has set down, is not true, but upon supposition, that the moon reflects all the light, it receives from the sun ; and yet his argument is drawn from comparing the light of the moon, seen in the day, with the light of the clouds, that

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(a) Lib. 3. prop. 58.

is, is deduced from the quantity of light actually reflected by the moon.

A S this gentleman at Art. 98th, &c. in relation to the apparent place and magnitude of the images of objects seen by reflection and refraction, has thought proper to depart from the principles delivered by the most approved writers on opticks; in order to form a judgment upon this innovation, I shall first set down distinctly the received opinion.

T H E figure and colours of objects are immediately presented to the sight by the images made upon the bottom of the eye; but their distance is judged of by concomitant circumstances. This amongst other means is naturally suggested to us by the rangement of the several objects, that occur to our sight; those appearing the most remote, where the greatest number of other objects are perceived to intervene. This way of receiving information of distance by the sight has been anciently taken notice of; being at large insisted on by Alhazen the Arabian in his treatise of Opticks, Lib. 2. n. 39. In extensive views there is also another more definite circumstance, well known to the painters, which suggests different degrees of distance. This is the blueish tint, which the colours of objects receive from the interposition of the air between them and the sight; which is greater in proportion, as a greater quantity of air is interposed between the eye and the object. But as the last of these two causes can have no sensible effect in very small distances, where the eye is found to form an exact judgment, even without the assistance of the other, it is supposed, that the eye is possessed of some powers within

within it self, whereby it receives this information. When any object is viewed attentively, we always turn the sight directly to it, that the axis of the eye be pointed to the object; and therefore in viewing an object with both eyes the axes of the eyes are more inclined to each other, when the object is near, than when it is remoter; and this being an animal action, is capable in consequence of habit of suggesting to the mind the different distances of objects by the different degrees of this conversion of the axes towards each other. Besides, as it is found, that the eye cannot see objects at the same time distinctly, which are at any considerable difference of distance, but can alternately fit it self to either; this change made in the eye is also by habit capable of suggesting distance. Both these causes are very well explained by James Gregory, in his *Optica Promota*. At the end of prop. 28, he thus expresses himself, “*quotidiana experientia majoris vel minoris contortionis [axium oculorum] natura est edocta, ut de distantia seu loco visibilis conjecturam facere possit;*” and of the second in the next proposition, “*naturæ insitum est, ut humores oculi fluidos & mobiles aliquo modo disponat ad radiantia propinqua, ut distincte pingantur in oculi retinâ; ex quorum humorum mutatione magna vel parva dignoscit natura (quotidianis experientiis edocta) num parvo vel magno intervallo distet radians ab oculo; atque ita de illius loco conjecturam facit.*” The first of these two causes was known before the latter, which was not apprehended till after the manner of vision by the refraction of the eye had been well considered; insomuch, that Kepler, who has explain’d the first of these causes distinctly (a),

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(a) Paralipomen. in Vitellionem, prop. 8.

confines the judgment of distance from a single eye to those distances, which bear a sensible proportion to the breadth of the pupil (*a*): and Aguilonius, who also admits of the first, lays down a proposition in form (though contrary to experience) that one eye only is incapable of judging of distance (*b*). That the first cause does operate to this effect, is from hence concluded, that we judge better of distance by both eyes than by one alone, as is usually experienced by directing a person to endeavour to touch an object at some small distance by a lateral motion of the finger with one eye shut. But it is also evident, that the single eye will likewise in some degree direct to the distance; because, if the object is not touch'd, the finger will not pass at a great distance from it; and those, who have the use but of one eye, will generally succeed in the experiment. In optick instruments, where the images of objects are viewed with one eye, this last cause is supposed to be that, which suggests the distance of those images; and that the eye sees them in that place, whence the rays proceed, which enter the eye from the several parts of the image.

THESE are the most direct means of our judging of distances. But as we do not always look at objects with the greatest attention, there are other circumstances accompanying objects, which offer themselves to our sight, whereby their distance is suggested. In particular the angle, under which an object appears, whose magnitude is supposed known, will give us an apprehension of a greater or lesser distance, according as that angle, or more properly the magnitude of the image

(*a*) Ibid. prop. 9.

(*b*) Optic. Lib. 3. prop. 1.

image made in the eye, is less or greater. Likewise if bodies are in motion, when the angle, under which they appear, increases, we shall judge the body to approach, or to recede, when that angle diminishes. These and other collateral means of our receiving suggestions of distance, when they do not conspire with the former, often occasion deception in the sight, and cause us to mistake the real distance of objects.

THE same deceptions are also liable to take place in the images of objects seen by optick instruments; and the experiments, by which this author was induced to depart from the received opinion, are evidently of this kind; where the image of the object by its increasing or diminishing upon the motion of the eye or glass seems to approach or recede, contrary to what it ought to do upon the common principles (*a*). But this gentleman not duly considering, that the eye was thus equally liable to deception in optic instruments, as in direct vision, has been so far confounded by these appearances, as to think it necessary to relinquish the common doctrine, and to advance in its stead a new rule for determining the apparent place of objects seen in those instruments.

ACCORDING to this gentleman the image of the object is always judged to be at that distance from the eye, where the object itself would be seen under the same apparent magnitude (*b*). But this rule is in the first place deficient; as it can be no guide to the eye, when the original object has not first been seen. It is also contrary
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(*a*) Art. 138. and the remarks upon it.

(*b*) Art. 139.

to fact in the most common and simple cases. In microscopes it is impossible, that the eye should judge the object to be nearer than the distance, at which it has viewed the object it self in proportion to the degree of magnifying. For, when the microscope magnifies much, this rule would place the image at a distance, of which the sight cannot possibly form any opinion, as being an interval from the eye, at which no object can be seen. Yet this gentleman expressly asserts, at article 141, that the apparent distance of the image of an object seen either by reflection or refraction, is to the distance of the object seen by the naked eye, reciprocally as the apparent magnitude of the object to the apparent magnitude of the image. I believe, in general, whoever looks at an object through a convex glass, and then at the object it self without the glass, will find the object to appear nearer in the latter case, though it be magnified by the glass; and in the same trial with a concave glass, though by the glass the object be diminished, it will appear nearer through the glass than without. But the most convincing proof, that the apparent distance of the image is not determined by its apparent magnitude, is the following experiment. If a double convex glass be held upright before some luminous object, suppose a candle, there will be seen two images, one erect and the other inverted. The first is made simply by reflection from the nearest surface, the second by reflection from the further surface, the rays undergoing a refraction from the first surface both before the reflection and after. If this glass has not too short a focal distance, when it is held near the object, the inverted image will appear greater than the other, and also nearer; but if the glass be carried off from the
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the object, though the eye remain as near to it, the inverted image will diminish so much faster than the other, that it shall appear at length very much less, but still it will appear nearer. Here two images of the same object are seen under one view, and their apparent distances immediately compared, and here it is evident, that those distances have no necessary connection with the apparent magnitude. This experiment will be rendered still more convincing by the following means. If the glass be of some considerable breadth, let a small bit of paper, or the like object, be stuck upon it somewhere about the middle. Then let the inverted image be viewed through a short tube, or a hole at such a distance from the eye, that the frame of the glass or any circumambient objects may be concealed from the sight. When the eye has attentively considered this image, and fixed an idea of its apparent distance, let the glass be gently moved, while the eye accompanies the image, till the paper come also in sight; then as the image is actually formed nearer to the eye than the glass, so the paper will evidently strike the eye with the sense of its being more remote than the image. If the same trial be made with the upright image, the paper will appear nearest. This experiment succeeds best, when the eye is held as near to the glass, as it can bear to see the images distinctly.

LET us now consider Mr. Professor's arguments, in art. 138. against the received opinion. One of his reasons, that the different divergency of rays cannot be the means of suggesting to us the place, whence those rays come, is, that " it " is a rational deduction from sense, which in- " forms us, that rays diverge from the points of " an

“ an object ; which the majority of mankind
“ are entirely ignorant of, and the ancient phi-
“ losophers, who thought, that something like
“ rays proceeded from the eye to the object,
“ could distinguish distances as well as we.” This
is quite as good a reason for the rays of light be-
ing no cause of our seeing at all ; because those,
who know the rays come from the object, and
those who imagined they issued from the eye,
could see equally well. But certainly, as the rays
of light make such an impression upon the eye,
as to raise in us the ideas of the objects whence
they proceed, though we have no immediate per-
ception of those rays ; so the change made in the
eye, in consequence of the different divergency of
the rays, may be a sufficient cause to suggest to us
difference of distance, without our having any di-
rect perception of that divergency.

ANOTHER argument is, that our sensible
ideas of the places of the remote parts of a long
walk or gallery, and of the clouds over head,
and of all celestial bodies, are quite different from
the rational ideas of the places, from whence the
rays diverge. But these are objects placed be-
yond the utmost distance of distinct vision, and
our not receiving information from this power in
the eye of the distance of objects placed beyond
its reach is no reason against its producing that
effect within the distances, at which it operates.

THE argument, drawn from the success of
painters in imposing on the sight by projections
in perspective, by the representation of lights and
shadows, and other parts of their art, will then
deserve to be considered, when a picture shall
be produced not distinguishable from the origi-
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nal object at a distance, in which the eye can see with perfect distinctness.

WHAT is said of objects seen through glasses by converging rays, is nothing to the purpose. For though we should admit this gentleman's opinion, that the appearance of their approach is to be ascribed to the augmentation of their visible magnitude, this can only be considered as one of the cases, in which the eye is subject to a deception from that cause.

HERE this gentleman has also objected against our forming a judgment of distance from the inclination of the axes of the eyes ; because these angles are varied by turning the head sideways, while we look at an object, though the distance remains the same. But as this is no proof, unless we saw things as perfectly by a side-view, as by a direct one ; so in his remarks (a) on this article he corrects himself, and upon second thoughts, it seems, will not dispute, whether the feeling of the turn of our eyes in directing their axes successively from a remoter object to a nearer may not also contribute to correct our judgment of its distance.

As our author has acknowledged for his predecessor in his opposition to the received doctrine a writer remarkable for the singularity of his opinions, and has even adopted his arguments ; I shall here occasionally observe, that that writer has in fact subscribed to the doctrine, he imagined himself to be opposing, in regard to the power of both eyes in this business. This is evident by comparing his words in the 17th section

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(a) Art. 245.

tion of his Essay towards a new Theory of Vision with the transcript, we set down above from James Gregory (*a*). As Mr. Professor has exactly followed the example of his original in assenting at last to the common opinion with regard to both the eyes, after having undertaken to oppose it; so possibly he might by his second thoughts have done the same in relation to one eye, had his author expressed himself somewhat more distinctly, where he has done so; for under the improper terms of straining the eye at Sect. 27, he has in reality acknowledged this consequence of the power, which fits the eye to each particular distance.

To apply this doctrine of apparent place to the assigning the apparent places of the images seen in optick instruments, it is necessary, that the apparent magnitude be assignable without regard to the apparent place, and for this end the antiquated doctrine contained in that ancient, but trifling piece of opticks ascribed to Euclide, that the apparent magnitude solely depends on, and is proportional to the angle, under which it is seen, is here at article 98 revived. Though Mr. Professor has delivered this maxim under the name of a definition; yet it is plain, he did not in-

(*a*) “ Because the mind has
 “ by constant experience (quotidiana experientia, &c. Opt. Prom. J. Gregor.) found
 “ the different sensations corresponding to the different dispositions of the eyes to be attended each with a different degree of distance in the object, there has grown an habitual or customary connection be-

“ tween those two sorts of ideas; so that the mind no sooner perceives the sensation arising from the different turn, it gives the eyes, in order to bring the pupils nearer or farther asunder, but it withal perceives the different idea of distance, which was wont to be connected with that sensation.

intend merely to impose an arbitrary sense upon the phrase apparent magnitude, but understood those words in their common acceptation for the idea of magnitude, which is excited in the eye upon the view of any object. Otherwise what is said in the 104th and following articles concerning the apparent magnitude of objects seen by refracted or reflected rays, would be little more than tautology. Besides, he afterwards, at article 156, follows prop. 6th of the treatise abovementioned in assigning the diminution of the angle (without taking in any additional consideration) as the reason, why the distant parts of parallel lines seem to the eye not so wide asunder, as the nearer parts. He also repeats after prop. 10th and 11th of this treatise the like reason, why the remote parts of a walk or a floor appear to ascend gradually, and the cieling to descend; and after Aguilonius, whom the authority of Euclide's name led into the same error, that the upper parts of very high buildings seem to lean forward over the eye below, and that the distant parts of a line extended from the eye diminish to the sight (*a*).

AFTER all this in the 160th and following articles Mr. Professor relinquishes this notion of apparent magnitude, and acknowledges, that it depends in part on our judgment of the distance; that two rows of trees which are parallel, by standing upon an ascent, whereby the more remote parts appeared farther off, than they really were, the trees were thought to diverge; that animals and all small objects seen in vallies contiguous to large mountains appear extraordinary small, because we think them nearer to us, than they really are; that in like manner, when

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things

(*a*) Optic. Lib. 4. prop. 2. & Consect.

things are placed upon the top of a mountain, or upon a large building, and are viewed from below, we think them extraordinary small for the same reason ; and in the last place, that the sun and moon in the horizon appear to us larger under the same angle, because they are judged farther off.

To reconcile these contradictions, in the remarks, art. 301, it is said, that there are two sorts of apparent magnitude. I hope, this gentleman does not mean, that objects have two apparent magnitudes at the same time : and if he only means, that in some circumstances objects give us the idea of one apparent magnitude and under other circumstances of another ; he ought to have shewn, how it can be brought to pass, that the idea of apparent magnitude, which is unalterable, either by the power of the imagination or any circumstances whatever, that do not affect the picture of the object upon the retina, can be obliterated, and another depending upon a collateral circumstance take place in its room.

A S this gentleman is very large upon the appearance of the horizontal luminaries ; and, by his referring at art. 333 of the remarks to Riccioli and a paper of Mr. Molyneux published in the Philosophical Transactions, Num. 187, for the history of what had been done before him, seems desirous, the world should be duly apprized, how much he has contributed to the explanation of that appearance ; these accounts being very defective I shall here give a fuller history of this matter. Ptolemy in his *Almagest* Lib. 1. c. 3. ascribes this appearance to a refraction of the rays
by

by vapours, which actually enlarge the angle, under which the luminaries appear; just as the angle is enlarged, by which an object is seen from under water. Thus he is understood by his commentator Theon (*a*), who explains distinctly how the dilatation of the angle in the object immersed in water is caused. This passage of Ptolemy we find copied by the two Arabian astronomers Alfraganus (*b*) and Geber (*c*).

BUT as soon as it was discovered, that there was no alteration in the angle, this cause was rejected. Accordingly we find, that Regiomontanus in his Epitome of the Almagest makes no mention of this cause assigned by Ptolemy, but refers to the optical writers (*d*), who agree in another opinion. Alhazen an Arabian author somewhat later than Alfraganus and Geber, though he admits, that some such refraction may at particular times cause a more than ordinary appearance of this phenomenon, yet he denies this to be the constant cause, and that the appearance is not owing to any real augmentation of the visual angle, but to an optical deception. After he has endeavoured to shew in distinct propositions, that the ordinary refraction of the air would rather diminish than increase the visual angle at the horizon; then to account for the appearance in question, he repeats from a former part of his work, that the sight judges of the magnitude of visible objects by comparing the visual angles, under which they are seen, with
their

(*a*) Pag. 10.

(*b*) Elem. Astron. cap. 2.

(*c*) Astronom. Lib. 2. pag. 21.

(*d*) In ea re sensum decipi
perspectivis conclamatum est.

Lib. 1. Conclus. 1.

their distances (*a*), and that, if the eye does not form a true judgment of the distances of objects, it will not form a just opinion of their quantities. Next he observes, that the sight perceives the colour of the sky, but not its form; and whenever the sight perceives any colour extended in length and breadth without discerning its true form, it will conceive it, as flat, by resembling it to the usual superficies, which occur to the sight, such as walls, or the like, and this is the appearance of all convex and concave surfaces at distances very remote (*b*): therefore that the sight apprehends the surface of the sky, as flat, and judges of the stars, as it would of ordinary visible objects extended upon a wide space; and when the sight views such objects dispersed over any extensive place, and sees them under equal angles, and perceives the quantities of their distances, then that, which is most remote, is esteemed the biggest (*c*). Again, as the sight
does

(*a*) Dico, quod in secundo tractatu hujus libri declaravimus, cum tractavimus de magnitudine: quod si visus comprehenderit magnitudines visibilia: comprehendit illas ex quantitibus angulorum, quos respiciunt visibilia apud centrum visus & ex quantitibus remotiorum, & ex comparatione angulorum ad remotiores. Optic. Lib. 8. n. 55. p. 280.

(*b*) Visus comprehendit colorem cœli, nec tamen certificat formam ejus nudo sensu. Et cum visus comprehenderit colorem aliquem in longitudine & latitudine: super hoc;

quod comprehendit figuram & formam: comprehendet ipsum planum: assimilabit enim ipsum aliquibus superficies assuetis, ut parieti & alijs. Et hoc modo comprehendit superficies convexas & concavas in remotioribus maxima. Ibid.

(*c*) Visus ergo comprehendit superficiem cœli planam, & comprehendit stellas; sicut comprehendit visibilia assueta separata, quæ sunt in locis spatiosis. Et cum visus comprehenderit aliqua visibilia assueta in loco aliquo spatioso, & comprehenderit illa angulis æqualibus, & comprehendit

does not perceive the concavity of the sky, and considers the stars, as placed in it, it perceives equal stars to be unequal, when in different places. For it compares the angle, under which a star is seen near the horizon, to a remote distance, and the angle, under which the same is seen in a more elevated part of the sky to a near distance, and estimates a star in or near the horizon to be greater than in the upper part of the sky ; and therefore estimates the same star in different parts of the sky to be of a different magnitude (*a*). And after having argued against the refraction of the air's being the cause of this appearance, because, though it conspired to this effect, it would be too small to produce the appearance, he sums up the whole thus : “ *Causa*
“ *ergo, propter quam videntur distantiae stella-*
“ *rum in horizonte majores, quàm medio cœli*
“ *aut prope: est illud: quòd sensus æstimat il-*
“ *las distare magis in horizonte, quàm in medio*
“ *cœli. Et hoc, quòd visus comprehendit stel-*
“ *las in diversis locis cœli diversas in magnitu-*
“ *dine: est error perpetuus: quia causa est perpe-*
“ *tua: & est: quoniam visus comprehendit su-*
“ *perficiem cœli planam, nec sentit concavita-*
“ *tem ejus & æqualitatem distantiae à visu. Ibid.*
pag. 282.

THIS

prehenderit quantitates distantiarum visibilium: tunc illud, quod est remotius, comprehendetur majus. Ibid. p. 281.

(*a*) Visus ergo comprehendit superficiem cœli planam, nec sentit concavitatem ejus, & comprehendit stellas separatas in ipso. Comprehendit ergo stellas æquales, separatas inæquales: nam comparat angulum, quem respicit stella extrema, propinqua horizonti

apud centrum visus, ad distantiam remotam, & comparat angulum quem respicit stella in medio cœli, & propinqua medio, remotioni propinquæ. Et similiter comprehendit stellam, quæ est in horizonte aut prope, majorem ea, quæ est in medio cœli aut prope. Comprehendit ergo eandem stellam & distantiam in diversis locis cœli, diversæ quantitatis. Ibid.

THIS cause of the appearance is so fully set forth by this writer, that he has been almost universally followed ever since. Vitellio assigns the same (*a*), and receives therein the approbation of his commentator Kepler (*b*); inasmuch that, where he seems to join the refraction of the air as an additional cause, Kepler ascribes it to a topographical error (*c*).

OUR countrymen John Peckham Archbishop of Canterbury and the famous Roger Bacon have very distinctly expressed the same. The former in a treatise entitled *Perspectiva Communis* first lays down this proposition, “*Distantiam horizontis majorem apparere quam alterius cujuscunque partis hæmispherii.*” His reason is, “*Ubi major magnitudo interjacere videtur, necesse est, ut etiam major distantia esse videatur. Sed inter horizontem & videntem tota terræ latitudo interjacere videtur. At inter videntem & punctum cœli verticale nihil interjacere videtur (d).*” Then his 82d proposition is this, “*Stellas in horizonte majores apparere, quam in alia parte cœli,*” which he proves from the former proposition, “*Quia magis distare videntur stellæ in horizonte, quam in alia cœli parte, ac tum in ortu, tum in medio cœli sub æquali angulo videntur; sequitur stellas in horizonte majores apparere quam alibi. Quia res ex æquali angulo ad majorem distantiam relata, major esse judicatur.*” The latter, though more concise is equally explicit. “*Quod visus judicat cœlum quasi planæ figuræ*
“*extensæ*

(*a*) *Optic. Lib. 1. c. n. 54.*

(*c*) *Ibid. pag. 132.*

(*b*) *Paralipom. in Vitell.*

(*d*) *Lib. 1. prop. 65.*

“ extensæ super caput in orientem & occidentem,
 “ quando aspicit ad alterum illorum. Sed quod
 “ videtur prope caput propinquius videtur, &
 “ ideo stella quando est in medio cœli videtur
 “ esse propinquior, & ideo in horizonte vide-
 “ tur magis distare, sed quod magis videtur
 “ distare, videtur esse majus, postquam sub eo-
 “ dem angulo videtur.” *Perspectivæ Distinct.*
 3. cap. 6.

THIS was the opinion of the cause of this appearance, while the Arabick learning prevailed, and the same has been generally adopted since. Besides Kepler before mentioned, who proposes again the same opinion in his excellent *Epitome of Astronomy*, Lib. i. pag. 81. Christopher Rothmannus somewhat earlier, being mathematician to the famous Landgrave of Hesse-Cassel, and contemporary with Tycho Brahe, at page 109 of his *Discourse on the Comet in 1585* in direct terms approves of the explanation of this phænomenon given by Alhazen. And Cardan still earlier has very distinctly expressed the same opinion in the following words. “ *Astra omnia, dum oriuntur & occidunt, majora videntur, quam in cœli medio, quoniam terræ magnitudo intermedia facit, ut oculus ea plus distare existimet, & ob id esse majora: nam & turris ulna major judicatur ab oculo illius distantiam comprehendente, tametsi minorem angulum in oculo faciat ulna ipsa (a).*”

AGAIN Martin Hortensius in his *Dissertation on the appearance of Mercury in the Sun*
 H anno

(a) *De subtilitate*. Lib. 3. ¶ 4.

anno 1631, at pag. 42 expresses himself to the same effect, "Quia cœlum longius a nobis
 " putatur distare in horizonte ob longissimum
 " terræ tractum interjectum, in medio au-
 " tem cœli videtur vicinius, quia nihil ei inter-
 " jicitur. Apparent quoque stellæ majores in
 " horizonte quam in medio cœli; cum eorum
 " quæ sub eodem angulo videntur, quæ longius
 " putantur abesse, majora appareant, quæ pro-
 " prius, minora."

A FEW years after, Des Cartes in his Diop-
 tricks cap. 6. p. 93. gives expressly the same rea-
 son; "hæc astra [sol & luna] circa meridia-
 " num in cœli vertice minora apparent, quàm
 " cum sunt in ortu vel occasu, & occurrunt inter
 " ipsa & oculos nostros diversa objecta, quæ ju-
 " dicio de distantia melius informant. Et a-
 " stronomi cum suis machinis illa dimetientes
 " satis experiuntur hoc, quod ita jam majora,
 " jam minora appareant, non ex eo contingere,
 " quòd modò sub majori, modo sub minori an-
 " gulo videantur, sed ex eo quod longius dista-
 " tudine judicentur."

THESE are the words of Des Cartes, which
 Mr. Molyneux in the paper, to which our au-
 thor refers his readers, has so strangely misrep-
 resented. Nor has Mr. Hobbes fared any better
 under his hands. For though he erred in suppo-
 sing, there was any necessary connection between
 the center of that arch, into which the sky ap-
 pears, as formed, and the center of the earth;
 yet he accounts for the appearance truly from
 our being to sense out of the center of that
 arch

arch(*a*); nay has extended this so far, as to observe this deception to operate gradually from the very zenith to the horizon, and that if the apparent arch of the sky be divided into any number of equal parts, those parts in descending towards the horizon will gradually subtend a less and less angle (*b*).

JAMES GREGORY also subscribes to the common opinion, though he agrees with Alhazen, that an extraordinary refraction by clouds may at particular times increase the appearance. *Geom. Par. Univers.* p. 141.

I WONDER our author should represent Mr. Molyneux's paper, as containing the last thoughts,

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which

(*a*) "Causa quare sol, luna,
" & cæteræ stellæ majores ap-
" parent prope horizontem
" quàm ab horizonte remo-
" tiores, ex eo oritur, quòd
" cum oculus sit in superficie,
" non in centro terræ magis
" distat ab horizonte aspecta-
" bili, hoc est, ab horizonte
" cæruleæ illius superficie
" quam vocamus cælum, quàm
" ab ejusdem summo culmine.
" Etsi enim terra ad orbem
" stellæ cujuscunque rationem
" magnitudinis habeat incon-
" siderabilem, magna tamen
" est comparata cum distantia,
" quæ apparet ab ipsa ad
" loca stellarum apparentia."

As this writer had very little mathematical skill, no wonder he should conceive the subject so indistinctly; but immediately after he gives a better reason for this difference in the apparent distance of the different parts of the sky.

"Accidit oculo he montem

"prospicienti distantiam ejus
" terram interpositam legen-
" do æstimare, unde magis
" videtur distare quàm a cæ-
" lo summo. Majorem au-
" tem distantiam apparentem
" sub eodem angulo visorione-
" cessario sequitur imago ma-
" jor." Hobbes de Homine
cap. 3. p. 17. If it be surpris-
ing, that this author should
here express himself in part
so ill, in part so justly, it may
be remembered, that he has
been suspected of receiving as-
sistance, in what he has writ
both in geometry and opticks
from the manuscript papers
of a person of more skill, Mr.
Warner the publisher of Mr.
Harriot's *Artis Analyticæ*
Praxis. See J. Wallisii *Elench.*
Geom. Hobbian. p. 116 &
Sethi Ward, *Exercitatio in*
Philosophiam Hobbianam p.
356.

(*b*) Hobbes de Homine,
cap. 7. p. 41.

which had been advanced upon this subject, excepting the fancy in the Essay toward a new Theory of Vision, which is here so diffusely examined; when in the same Philosophical Transaction, in which Mr. Molyneux's paper was published, there follows a discourse of Dr. Wallis, which also is quoted in the New Theory of Vision, wherein the Doctor assigns the same cause as those authors, we have here named. And since Mr. Professor thought fit to examine in form so trifling an opinion, he should not, methinks, have put us off with a bare referring to Riccioli for the history of the subject. For he has made mention of no other than the two opinions, which we have already taken notice of, excepting that of Gassendus; whereas Baptista Porta has given us no less than three others (*a*), and Scheiner has likewise suggested a particular thought upon the subject, that the appearance might in part arise from the contraction of the perpendicular diameter of the luminaries, or the perpendicular distance of stars near each other, whereby we may be led into a wrong judgment, and instead of considering these, as lessened, conceive the horizontal diameters of the luminaries, or the horizontal distance between two stars, as increased. Refract. Cœlest. cap. 28. pag. 46.

BUT indeed none of these obscure opinions deserve any particular examination; nor even that of Gassendus, though an author of greater fame, who would ascribe the appearance merely to a dilatation of the pupil, which he supposes to attend the view of celestial objects near the ho-

(*a*) De Refract. L. 1. prop. 12.

horizon (*a*); for this is inconsistent with the principles of opticks, and the author of it so totally ignorant in that science, as to assert, that a dilated pupil magnifies an object for the same reason, as a convex glass does (*b*). His letters upon this subject contain likewise divers other absurdities; that the office of the crystalline humour is to erect the images of objects, which otherwise would appear inverted (*c*); that no object is ever seen by both the eyes at once for this strange reason, that because an object viewed first with one eye alone, and then with the other, appears to cover different places of a distant wall, therefore if the axis of both eyes were together directed to the object, both those parts of the wall, if not the whole interval, ought to be hid (*d*), nay, great part of those letters is taken up in reviving and justifying the ridiculous Epicurean scheme, that objects are seen by species continually flying off from them.

NOR do we find, that this opinion of Gassendus gained any credit with the skilful, however a writer or two of little note may have fruitlessly endeavoured to make sense of it. On the contrary Huygens in his treatise upon the Parhelia, translated by our author, has approved, and very clearly explained the received opinion (*e*).

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(*a*) De apparente magnitudine solis humilis & sublimis Epist. 1. art. 5.

(*b*) Ibid. Epist. 2. art. 13. pag. 39.

(*c*) Ibid. art. 4. p. 18.

(*d*) Ibid. art. 17. pag. 52.

(*e*) "The cause of this fallacy in short is this, that
 " we think the sun or any
 " thing else in the heavens
 " to be remoter from us,
 " when it is near the horizon,
 " than when it approaches
 " towards the vertex, because
 " we

THIS apparent greater distance of the sky near the horizon is so very obvious to the sense, that we find it assumed, as indubitable, upon all occasions. Aguilonius, though he does not consider the effect of this appearance upon the luminaries, or the distance of stars, has in the very page quoted by our author, in article 160, for another purpose this proposition, "*Cœlum prope horizontem*
" longius a nobis distare videtur quam juxta ver-
" ticem (a). And Mersennus in his Opticks, pag. 495, says, "*Vifus non deprehendit quan-*
" tum astra distant a nobis, & cœlum terræ in
" ambitu horizontis cohærere putat : res enim
" ut plurimum propinquiores existimantur, qua-
" rum intermedium spatium non percipitur :
" hinc cœlum prope horizontem longius quam
" juxta verticem a nobis distare videtur."

BUT Mr. Hobbes is the only one, who has expressly considered this vaulted appearance of the sky, as the real portion of a true circle, and Mr. Professor has followed him herein so far, as to improve upon his thought of dividing this arch into equal parts ; proposing to find what portion
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" we imagine every thing in
 " the air that appears near
 " the vertex to be no farther
 " from us than the clouds
 " that fly over our heads ;
 " whereas on the other hand
 " we are used to observe a
 " large extent of land lying
 " between us and the objects
 " near the horizon, at the far
 " end of which the convexi-
 " ty of the sky begins to ap-
 " pear ; which therefore with
 " the objects that appear

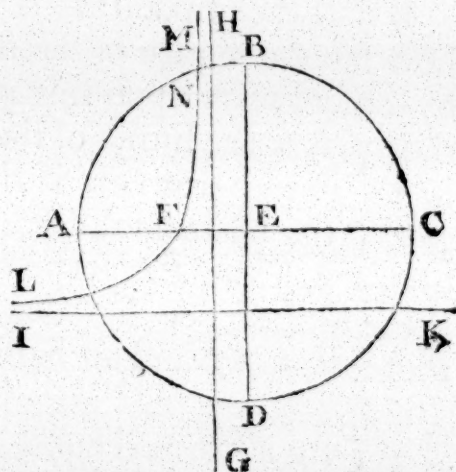
" in it is usually imagined
 " to be much further from
 " us. Now when two ob-
 " jects of equal magnitudes
 " appear under the same vi-
 " sual angle, we always judge
 " that object to be larger
 " which we think is remoter.
 " And this is the true cause
 " of the deception we have
 " been speaking of." Art.
 556.

(a) Optic. Lib. 4. Prop. 4.
 Consect. 1.

of a circle this arch contains by observing the angle of the elevation of the middle point between the zenith and horizon.

As this is the only addition, this gentleman has made, to what had been before advanced upon this subject, I am surprized to find the problem so very slightly passed over, since it is capable of a geometrical solution, which affords a better mechanical construction than the inartificial one, he acquiesces in ; and besides admits of a computation so concise, as to be preferable to either.

If in the circle ABCD, whose center is E, two Diameters AC, BD be drawn perpendicular to each other, and in AE, EF be taken to AE, as the tangent of the angle observed to the radius, this angle being less than half a right the point F will fall



between A and E. Then E F being divided into two equal parts by the line GH parallel to BD, and ED also divided into two equal parts by the line IK parallel to AC, and to the asymptotes GH, IK the hyperbola LM being described through F cutting the quadrantal arch AB in N; the arch BN will be similar to half the apparent arch of the sky between the zenith and horizon. But the problem may also be solved by computation thus. Deduct the square of the tangent of the angle observed, and one third of that square each from

from the square of the radius. Then, as the radius is supposed unity, the arithmetical complement of the logarithm of the greater of these two numbers, half that complement, and the logarithm of the other number being added together give the logarithmick cosine of an angle, the cosine of the third part of which exceeds half the arithmetical complement of the greater of the two forementioned numbers by the sine of the angle, whose tangent is to the radius, as the apparent horizontal distance to the apparent perpendicular distance.

By this computation it will be easy to judge, how far the figure of the sky can be determined by observation upon this principle. As our author places the limits of the middle altitude within 18 and 30 degrees, if the altitude be 20 degrees, the horizontal distance will be scarce less than four times the perpendicular distance; but if the altitude be 28 degrees, it will be little more than two and an half.

AFTER this account of what authors have said upon this phænomenon, it plainly appears, how little reason Mr. Professor has to assert in article 333 of his remarks, that ever since the time of Ptolemy the cause of it has been dubious and disputed. Probably he scarce looked farther into this matter than the dissertation of Mr. Molyneux, he here praises, who also for want of inquiry and mistaking the authors, he had seen, had thus represented it. But we find on the contrary, that from the time of Alhazen the opinion of the best writers had been, as it were, unanimous. Where this gentleman learnt, that the
first

first astronomers were not aware, that the intervals of stars seem much greater near the horizon than the meridian, I cannot conjecture : for surely it is impossible to have lookt at the stars without perceiving it. Nor can I find, where Ptolemy has given that piece of advice, our author here ascribes to him. In the place, he has quoted, nothing is said upon the subject. But I suppose, he intended to refer to the second chapter of the ninth book of the *Almagest*, which is cited by Kepler on this occasion (*a*), though nothing farther is there to be found than this short observation, that the difference in the apparent distance between stars in different elevations created a difficulty in judging of the exact time, wherein the planets are seen stationary.

IN this particular inquiry, besides shewing the true state of the subject, I had also a farther design, to set forth how well the principles concerning the apparent magnitude of objects had been established, and how generally approved, which this gentleman has rejected for that crude conception concerning it, advanced in the first dawn of this science. But it will not be amiss to see farther how distinctly optical writers have expressed themselves upon this head in general.

ALHAZEN has here delivered himself in very distinct terms, that though the apparent magnitude of objects had been by most ascribed to the magnitude of the visual angle only, yet that this opinion was undoubtedly false. He assigns this reason, that within moderate distances it is evident to the sense, that the object does not appear less to the eye by increasing the distance. That for instance

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(*a*) *Paralipom. in Vitel. pag. 134.*

the eye does not perceive a thing at two cubits distance, as if it were less, than at the distance of one cubit, nor even tho' it were remov'd to the distance of three or four (a). He confirms this farther by observing, that if two diameters are drawn in a circle, and the circle so presented before the sight, that one diameter shall appear under a less angle than the other; or if the two sides of a square are offered to the sight in the same manner; yet, if the distance be moderate, the eye shall notwithstanding see one as a circle, the other as a square (b). To shew how considerable a power the distance of the object has upon the idea, we receive from the eye of its magnitude, he afterwards proposes this observation; if a man should cover
with

(a) Plures opinantur, quòd quantitas magnitudinis rei visæ non comprehenditur a visu, nisi ex quantitate anguli, qui sit apud centrum visus, quem continet uerficies pyramidis radialis, cujus basis continet rem visam: & quòd visus comparat quantitates rerum visarum ad quantitates angulorum, qui sunt a radiis, qui continent res visas apud centrum visus, & non sustentatur in comprehensione magnitudinis, nisi super angulos tantum. Et quidam illorum opinantur, quòd comprehensio magnitudinis non completur in comparatione ad angulos tantum, sed per considerationem remotiois rei visæ, & situs ejus cum comparatione ad angulos. Et veritas est, quòd non est possibile, ut sit comprehensio quantitatum rerum visarum a visu ex

comparatione ad angulos, quos res visæ respiciunt apud centrum visus tantum. Quoniam eadem res visa non diversatur in quantitate apud visum, quamvis remotiones ejus diversentur diversitate non magna. Quoniam quando res fuerit prope visum, & ipse comprehenderit quantitatem ejus: & postea fuerit elongata a visu non multum: non diminuetur ejus quantitas apud visum, quando ejus remotio fuerit mediocris. — non comprehendit visus rem visam in remotione duorum cubitorum, minorem, quam in remotione unius cubiti. Et similiter si elongetur à visu per tres cubitos aut quatuor, non videbitur minor, quamvis anguli, qui sunt apud visum, diversentur diversitate extranea. Opt. Lib. 2. n. 36.

(b) Ibid.

with his hand before his sight a great part of^a distant wall, or of the sky, and then upon the removal of his hand observe the idea raised in his mind from that part of the wall or sky, which his hand before covered, he will perceive his imagination to be affected in no degree with the impression of his hand being of a like apparent magnitude with that part of the wall or sky, it covered (*a*). This is so distinctly explained by this author, and so generally followed (*b*), that Herigone has set it down as an optical axiom, “*Quæ sub eodem angulo videntur, quæ longius putantur abesse, majora apparent* (*c*).”

THIS point therefore having been so generally received, I think Mr. Professor’s fellow-writer might have spared himself the trouble of his long dissertation in support of this opinion (*d*); which can only serve to persuade the world, that these gentlemen had but just come to the knowledge of so common a principle.

WHEN Alhazen has expressed himself so distinctly upon this subject, as we have seen, it is a strange assertion of Tacquet, that every one of the ancient writers upon opticks supposed the visual angle alone to determine the apparent mag-

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nitude

(*a*) Ibid. n. 38.

(*b*) See Vitell. L. 4. prop. 27. Peckham Perspec. L. 1. prop. 64. Bacon Persp. Distinct. 3. c. 5.

(*c*) Cursus Math. Tom 5. pag. 15. This author conceived so little difficulty in the appearance of the horizontal moon, that he produces it as an indubitable proof

of the axiom. “*Ut luna, quamvis in horizonte & medio cœli, sub eodem angulo cernatur, major tamen in horizonte quàm in medio cœli apparet; quod ejus distantia in horizonte major existimetur propter interjecta corpora.*”

[(*d*) Art. 171. of the Remarks.]

nitude (*a*). But this author can be of no account in this matter; since he had so little considered the operation of the eye, as to subscribe to the absurd assertion already mentioned of Gassendus, that the axes of both eyes are never directed to the same object (*b*).

Aguilonius, though he adopts the same form of expression, yet by better considering the subject found himself obliged to acknowledge, that the eye does in fact estimate the magnitude of objects by comparing the visual angle with their distance: “Vim oculis natura concessit, qua
“ simul rei distantiam percipiant, atque ex ejus
“ collatione cum angulo pyramidis opticae, veram magnitudinem quam proximè dignoscant.
“ Manifestè enim deprehendimus errores omnes, qui in magnitudinis perceptionem irrepunt,
“ ex distantiae ignoratione originem ducere (*c*).” And he afterwards qualified the assertion, that those things, which appear under the same angle, are judged equal, and those, which appear under the greater angle, greater, by limiting it to the case, when the difference of distance is not taken cognizance of (*d*).

WHAT Aguilonius has mentioned of the errors we fall into in relation to the apparent magnitude

(*a*) Quæ sub æquali angulo videntur, apparent æqualia; quæ sub majori, majora; minora sub minori; & qualis est angulorum opticorum, talis est & magnitudinum apparentium proportio. Id veteres optici ad unum omnes axiomatis loco habuere. Opt. Lib. 1. prop. 3.

(*b*) Ibid. prop. 2.

(*c*) Optic. L. 3. prop. 11.

(*d*) PROP. X.

Majoribus spectata angulis majora, minora minoribus, æqualibus æqualia videntur.

In hac propositione subintelligendum esse, ut ratio disparis intervalli, quo magnitudines ab aspectu distant, penitus ignoretur. Opt. Lib. 4.

nitude of objects being caused by a wrong judgment of their distance, Alhazen has particularly insisted on, when amongst the several causes of errors in vision, he sets down one to be great distance, and shews, that from thence will arise an error in our judging, both of the interval between distant bodies, and also of their magnitude (*a*).

IT is this error in our judgment of distance, which causes parallel lines to seem converging, and the like deceptions, which in the optical treatise ascribed to Euclide, and by our author are referred to the mere decrease of the angle. Trees are sometimes planted so as to appear in one point of view, as parallel. But in these the angles subtending their distances continually diminish, as these distances are farther removed from the sight, though not so much, as when the trees are really parallel. If the angle only determined it, trees, which extended in two lines from the point of view, must be seen as parallel.

BUT now to sum up the result of this disquisition concerning the apparent magnitude of objects; as we have shewn the concurrent opinion of all the best writers to be, that it is connected with our ideas of their distance; so this gentleman has not only admitted as much in many objects viewed by the naked eye; but also in terrestrial objects seen through telescopes (*b*), that the object does not strike the beholder with the appearance of its magnitude being increased as much as the angle, under which it is seen, because it is thought nearer. And this is certainly the true cause of the appearance. But I am very
much

(*a*) Optic. Lib. 3. n. 52. (*b*) Remarks Art. 243.

much surprized to see it affirmed, that, what is here said, expresses the true sense of his definition of apparent magnitude given in the 98th Article of his book, where it is asserted, that the greater visual angle not only causes a larger picture of the object on the bottom of the eye ; but that this picture being larger or smaller causes a sensation of a greater or smaller visible extension.

IN short, what has perplexed Mr. Professor is this, that the apparent magnitude of very distant objects is neither determined by the magnitude of the angle only, under which they are seen, nor is in the exact proportion of that angle compared with their true distance, but is compounded also with a deception concerning that distance ; insomuch that if we had no idea of difference in the distance of objects, each would appear in magnitude proportional to the angle, under which it was seen ; and if our apprehension of the distance was always just, our idea of their magnitude would be in all distances unvaried : but in proportion as we err in our conception of their distance, the greater angle suggests a greater magnitude. Our author not being apprized of this compound effect, has sometimes explained the apparent magnitude by the visual angle only, at other times by that angle compared with the distance, and by that means becomes thus inconsistent with himself.

HOWEVER besides this gentleman's inconsistency, the method he has taken to determine the place of images made by reflection or refraction simply from the magnitude of the angle, under

under which they are seen (*a*), is certainly erroneous. For not to mention other examples to the contrary, when the inverted image made by reflection from a convex lens appears less than the erect image; though the inverted image seems nearest, yet it not only looks less to the sense, but also in reality subtends a smaller angle, as may be easily proved by bringing one of the images over the other.

I BELIEVE, I need not apologize for the length of my remarks upon this point, because it relates to a principle, that extends itself throughout this whole work, and in which Mr. Professor triumphs as a great discovery in opticks unknown even to Sir Isaac Newton or Mr. Cotes (*b*). But I shall now conclude this head with one short remark upon, what he has said concerning the inventors of the telescope. For, as he has promised us the historical, as well as the other parts of the subject, this account would have been less deficient, if besides the translation of Huygens's history of the invention, and the descant he has made upon Mr. Molyneux's claim in behalf of Roger Bacon, he had taken notice of the pretensions of another of our countrymen Leonard Digges Esquire. For if Roger Bacon had never compounded glasses together, it is certain, this gentleman had. For he expressly says in the 21st chapter of the first book of his Pantometria, published by his son Thomas Digges Esquire in 1571, "Marvellous are the conclusions that may
" be performed by glasses concave and convex
" of circulare and parabolicall formes, using for
" multiplication of beames sometime the aid of
" glasses transparent, which by fraction should
" unite

(*a*) Artic. 139.

(*b*) Remarks Art. 197, 465, 494, 536.

“ unite or dissipate the images or figures presented by the reflection of other. By these kinds of glasses or rather frames of them, &c.”
 Whoever reads the whole chapter, I think, can scarce doubt, but the author had seen in some measure the effects of viewing an object made by reflection from concave or convex surfaces through a lenticular glass.

HOWEVER, if Roger Bacon were quite ignorant of the telescope, yet I cannot agree, that the use of glasses of small convexity for the relief of the sight decayed by age was so very imperfectly known by him, as this gentleman supposes (a); for the more deficient or erroneous his theory is, the less probable is it, he should make such an assertion in terms so express (“ hoc instrumentum est utile senibus”) if he had not seen the effect. The cursory manner, in which this is mentioned, persuades me, it was not an invention of his own; but I cannot conceive, how he should have said it so directly, if this use of glasses had not at that time begun to be in practice; especially in a chapter, which professes to exhibit examples of the doctrine before laid down. Mr. Professor’s opinion, that the friar had here only in his thoughts a plano-convex glass with the flat side laid immediately upon the object, I cannot subscribe to. Not to examine into the criticism on the word “ suppositi”, the consequence drawn from Roger Bacon’s considering in his reasoning only the refraction of one surface is, I think, certainly invalid: for by the same argument one might conclude, that telescopes are now most usually made with a plano-convex object-glass; because Dr. Smith has determined the relation of their lengths
 to

(a) Remarks Art. 88.

to their apertures, charges, and power of magnifying upon that supposition (*a*). Certainly since Mr. Professor in so ample a treatise has thought fit to conclude these points from the case, where one refraction only takes place; though the object-glasses of those instruments in their common fabrick have two surfaces, which refract; why may we not suppose this old writer in that very imperfect state of the science to have taken the like liberty.

IN what relates to the philosophical theory of light, this gentleman has had the caution to set down Sir Isaac Newton's discoveries for the most part in his own words (*b*); but has entirely omitted all, that he has established in his second book of Optics concerning the cause, why some part of the light is transmitted, and some part reflected at every pellucid substance. Now as this speculation is the strongest instance of the wondrous capacity of that great man in unravelling the most intricate and disguised operations of nature, and establishes at the same time such a series of properties belonging to light, as give us no small reason to hope, that by diligently following the clue, he has here put into our hands, still greater secrets of nature may one day be brought to view; Mr. Professor ought not to have passed over so important and complicated a subject by a bare hypothesis, that the passage of a ray of light at a surface, where it is reflected, is in a curve with one point only of contrary flexion at the very surface (*c*). For if the light is inflected in its passage near the edges of bodies by the same principle, as it is reflected and refracted, and the alternate returns of easy re-

K

flexion

(*a*) B. 2. chap. 6. & 7.(*c*) Article 191.(*b*) B. 1. chap. 6, 7, 8.

reflection and easy transmission will give the light in its passage by those edges so many different bendings as to form three or even more visible fimbriæ to the shadows of bodies, why may not the same alternate dispositions at refracting surfaces produce in the light a more compound motion?

IN the remarks upon the cause of refraction and reflection, since Mr. Professor thought proper to explain the hypothesis of Leibnitz (*a*) more at large than any other, it would not have been amiss for him to have ascribed it to its true author, Mons. Fermat (*b*), nor would it, I believe, have displeased a geometrical reader in so large a work, containing so many collections, to have seen Huygens's most elegant demonstration from his treatise of light (*c*).

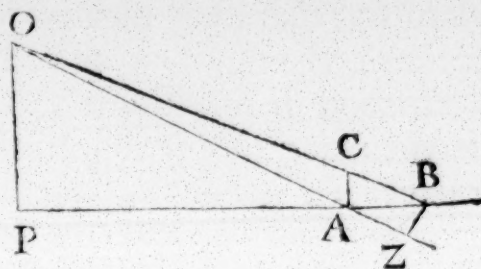
I SHALL now proceed to consider the 2d book of this treatise, in which we may discover perhaps one cause of Mr. Professor's falling into the abovementioned mistakes. For here, I think, appears throughout that inexpertness in demonstration, which shews him to have been too little conversant in the writings of the best geometers. Of this we might have before taken notice of some examples. For instance, in article 157 it is said, that the apparent magnitude of a given line AB seen very obliquely at a given

(*a*) Article 413.

(*b*) See *Varia Oper. Math.*
Pet. Fermat, pag. 156, &c.

(*c*) *Traité de la Lumiere*,
chap. 3. p. 40.

ven distance OA
(meaning the
angle AOB,
which it sub-
tends at the eye
in O) increafes
and decreafes in
proportion to



the increafe or decreafe of OP, the perpendicu-
lar diftance of the eye from the line AB produ-
ced; provided that the diftance AO be very
large in comparifon to AB. But though this af-
fertion may be admitted, his proof is not to be
juftified. Firft, he fays, if the ray BO cut a
line AC perpendicular to AB in C, while the
eye is raifed or deprefsed in the perpendicular
OP, the line AC will increafe and decreafe, as
OP does: whereas this is falfe; for AC is to
OP as BA to BP, which is not a given propor-
tion, fince neither BP nor AP are given, but
AO. Again the angle AOB does not fo pro-
perly vary in the fame proportion with the line
AC, as with a line let fall from A perpendicular
on OB, or from B perpendicular on OA; and it
is this perpendicular BZ, which is in a given
ratio to OP, BZ being to OP as AB to AO.
From this wrong turn given to the demonftra-
tion he was obliged to put an unneceffary re-
ftriction upon the propofition; for it is equally
true, when the diftance AO bears a very great
proportion to AB, whether AB be feen with a
great or fmall degree of obliquity.

BUT in the fecond book we find perpetual
proofs of this unskilfulnefs in regard to geome-
trical demonftrations. Hence his complaint a-
gainft the great Huygens's demonftrations for

being tedious, intricate and embarrassing to the reader, by what he is pleased to call formal compositions and resolutions of ratios (*a*), the inartificial method of marking the same points with different letters (*b*), demonstrating by folding up the paper (*c*), grounding his demonstrations (*d*) concerning caustics upon the description of them in the popular part of his treatise, where eye-sight is appealed to for the light being condensed at them, the unusual phrases of proportionable, middle proportional, conjointly to signify every method of adding the terms of proportionals, and disjointly both for division, conversion, and taking the difference of the antecedents and of the consequents. To the same cause was owing the great inaccuracy, with which he expressed himself in relation to what he calls the center of a lens. This likewise could be the only reason, which should induce him to take refuge in algebraical calculations (*e*), in a case, whose demonstration is no more than this. XY being to TX as AP to PT , and $X\tau$ to XY as TP to $P\beta$, by equality $X\tau$ is to TX as AP to $P\beta$, and by composition $T\tau$ to TX as $A\beta$ to βP , or as $B\beta \times \beta A$ to $B\beta \times \beta P$. Again TF being to $F\tau$ as APq to $P\beta q$, by conversion TF is to $T\tau$ as APq to $A\beta \times \beta B$. Therefore $T\tau$ being to TX as $A\beta \times \beta B$ to $P\beta \times \beta B$, by equality TF is to TX as APq to $P\beta \times \beta B$. Consequently TX is greatest, when the rectangle under $P\beta B$ is greatest, that is, when it is equal to one fourth of the square of PB or AP , and then, since TF is to TX as APq to $P\beta \times \beta B$, TX will be equal to a fourth of TF , and XY equal to one fourth of FG .

OUR

(*a*) Remarks, Art. 421, 475.(*d*) Art. 329.(*b*) Fig. 440 & 443.(*e*) Art. 335.(*c*) Art. 214.

OUR author seems himself conscious of this imperfection, when he takes advantage from his subject to express the lemma, whereon Sir Isaac Newton builds his demonstrations by prime and ultimate ratios in such terms, as he thinks, may be allowed in a physical subject, but in a demonstration purely mathematical have certainly no meaning. For, when it is said, that quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last, were he asked to explain, what that state is, which can be understood immediately to precede the last in any subject of geometrical demonstration, it were impossible to make sense of the question. Nay, so apprehensive is he of a defect in his geometrical reasoning, that he even refers to computation (a method, he has in another place (a) censured) as an additional confirmation (b).

BUT besides such imperfections we find several direct errors. The fourth chapter has affixed to it a very pompous title, though it contains no more than two propositions, and those relating to the very easiest cases. The second of which gives

(a) Remarks Art. 421.

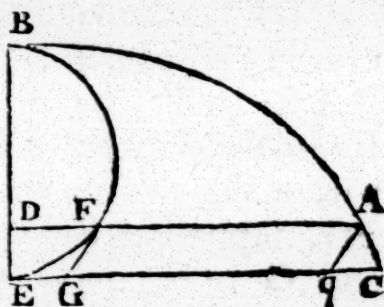
(b) The whole lemma runs thus, "Quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last; and also in a state somewhat remote

"from the last without sensible error in physical subjects: and the same may be said of figures which continually approach to a state of similitude; especially if these errors, when computed, are found inconsiderable. Art. 204.

gives only an approximation except in the sphere. The first is scarce true in any kind of object besides a plane or curve line parallel to the refracting surface. It is here only proved, that the distances of any intermediate point of a streight line from the extremes are in the same proportion in the image, as in the original. But streight lines are never in geometry said to be similar, because they are proportionally divided. If this term is to be applied to the right line, it must be said without restriction, that all streight lines are similar. In every other object not parallel to the refracting surface, the proposition is directly false. If the object were the arch of a circle in any plane not parallel to the plane of refraction, its image, as here assigned, would be elliptical. It is also false in every plane object, not parallel to the refracting surface ; and much more so in every solid object.

INDEED the demonstrations in the first chapter of the foci, besides the imperfection of the principle, upon which they are grounded, are on another account also inconclusive. As Dr. Barrow has searched with much more exactness than any, who had gone before him, into the place, where rays are collected by reflection or refraction, and where the images of objects formed by this means are seen ; so his exact inquiry into the line, in which each ray passes, and in what degree they condense together in every circumstance drawing out his disquisition into some length, others have endeavoured to give a more concise, though less perfect idea of this subject. For this purpose, in particular, Dr. Gregory in his Elements of Opticks has by a method of demonstration similar to that of our author content-
ted

ted himself with shewing, whereabouts those rays would cross the axis of a reflecting or refracting spherical surface, which would enter the eye placed in that axis; and thus far his demonstration may be admitted as just. But our author having used the same method to assign the place of the focus independent of the eye; his demonstrations are by no means so free from exception. For in the first part of his work (*a*), he has always represented as necessary to constitute a focus, that the rays be in that place condensed together; but his form of demonstrating may be applied to cases, where there is no condensation of rays. For example, if rays parallel to the base of the cycloide BAC generated by the revolution of the semicircle BFE upon the line EC be reflected, as the ray DA is reflected into Aq, as the point of incidence A approaches towards C, the lines Eq, EC continually approach toward equality, and become equal, when the distance DE vanishes; yet here is no condensation of the rays on the axis at the point C; because the intervals between the points *q* appertaining to equidistant rays increase, while DA approaches EC. This is evident, because Aq is parallel to FG, the tangent to the semicircle BFE at the point F, where the rays intersect that semicircle, and Cq is equal to the excess of the arch EF above the tangent.

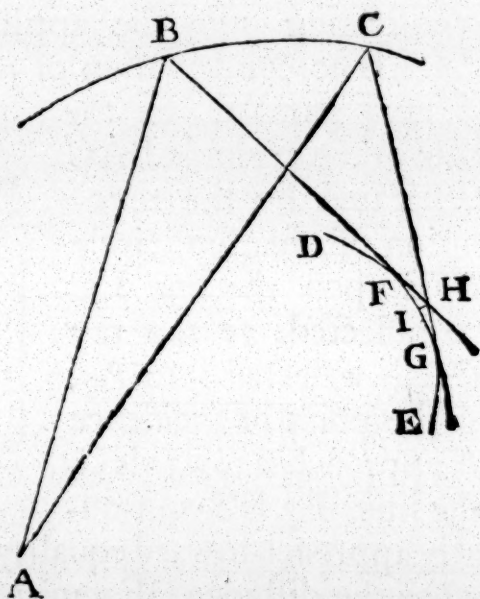


AR-

(*a*) Book 1. chap. 2.

ARTICLE 468 contains a false conclusion from the preceding principles. For the density of the light, which is proportional to its heat or warmth, is to be estimated by a perpendicular section of the reflected rays, and to be determined by the aberration of the rays from the point of contact at the caustic after this manner.

If rays diverging from any point A are reflected by the surface BC into the caustic DE, all the rays in the pencil BAC will be spread through the part of the caustic FG, supposing F and G to be the points, in which the reflected rays BH and CH touch the caustic. Here



the density of the light upon the caustic must not be measured by the magnitude of FG, but by a perpendicular section of those rays, where they are most condensed. Thus if HI be drawn from H perpendicular to the curve, every ray of the pencil BAC will pass through that line HI. This line then will nearly represent the magnitude of a perpendicular section of those rays at I; and therefore if two pencils proceeding from A are taken so that the quantity of light, or, which is the same, that the angles made by

by the extream rays may be equal in each, the densities of the light at the middle of the parts of the caustic formed by each pencil will be reciprocally as the line IH in each. But IH is ultimately the chord of $\frac{1}{4}$ the angle BHC to the radius GH or GI. The density then estimated by this means will be reciprocally as GI, or its double GF compounded with the reciprocal of the angle BHC.

HENCE it follows, that when to equal angles made by the incident rays at A unequal angles are made by the reflected rays at H, the density of the rays will not be justly estimated by the reciprocal of the part FG of the caustic included between the reflected or refracted rays. For instance, though in our author's case, at art. 465. when the rays reflected by a circle issue from a point in its circumference the density of the light in the superficial caustic, which is proportional to its heat, is, as this gentleman has computed, reciprocally as $bH \times HI$: yet in his 467th art. when the rays are parallel, this density is not in the simple ratio of BD directly and the triplicate of CD inversly, as he has determined; but in the duplicate ratio of DB directly and in the triplicate of CD inversly.

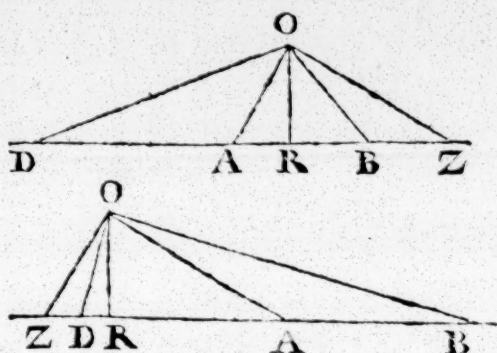
WHEREAS this gentleman professes to have rendered the doctrine of the rainbow more general, than it has been yet handled; all that he has added to what Dr. Halley had in the Philosophical Transactions Num. 267. already done upon that subject, is the applying to the computation of the inverse problem a general theorem of Mr. John Bernoulli published in the Acta Eruditorum (a), for finding from the tan-

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gent

(a) Ann. 1722. Mens. Jul.

gent of an arch the tangent of any multiple of that arch. The lemma in article 508, from which this general theorem is deduced, should have been thus demonstrated. If ROA be one angle, ROB the other, and ROD their sum or difference.

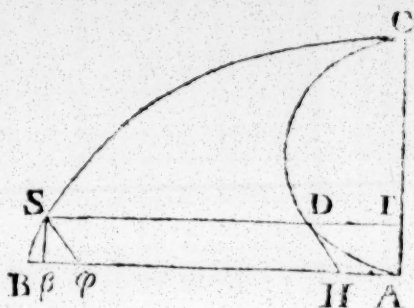


Draw OZ perpendicular to AO , then the angle

ROZ is equal to the angle RAO , and therefore the angle ROB being equal to the angle AOD , that under RDO is equal to that under BOZ ; whence as DZ to OZ so is OZ to BZ , but as OZ to ZR so is AZ to OZ , so that by equality, as DZ to RZ , so is AZ to BZ , and by division as DR to RZ so is AB to BZ , or by permutation DR is to AB as RZ to BZ or as $RZ \times AR$ to $BZ \times AR$, that is, as ORq to $ORq - AR \times RB$, when AB is the sum of the given tangents and RD the tangent of the sum of the arches, but as ROq to $ROq + AR \times RB$, when AB is the difference between the given tangents, and RD the tangent of the difference of the arches.

THE proposition contained in article 602 is not universally true, but fails, whenever the curvature at the vertex of any one of the curves cannot be compared with that of the circle. For
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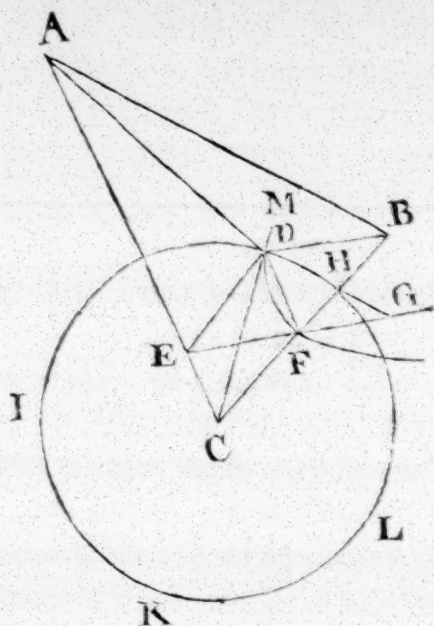
instance, in the cycloide ABC, when any ray parallel to the base AB is reflected at S, B ϕ , the aberration from B, is equal to the arch AD lessened by the tangent DH or HA,



and B β is equal to AD—DI; but AD—AH bears not ultimately a finite proportion to AD—DI. And hence the corollary in article 603 is not true, when one or more of the surfaces are cycloides or curves of the like curvature.

BUT the first proposition of this chapter is both defective and erroneous upon the author's own principles. The place of the image cannot be determined, as is here required, till it be shewn how to find the line, in which it is seen, that is, how to assign the reflected or refracted ray, which shall enter the eye. But of this our author has been absolutely silent. The problem is indeed in most cases too complex to admit of a simple and elegant solution; but this is no excuse, why in a treatise promis'd to be compleat, no notice should be taken at least of what had been done by others in this particular. Alhazen's problem to find the ray, which issuing from a given point should be reflected by a spherical surface to the eye in any given position, was thought worthy to exercise the invention of two of the principal mathematicians of the last age. The hyperbola, by which Huygens solved the problem, especially as determined by Slufius in the Philosophical Transactions, N^o. 98. admits

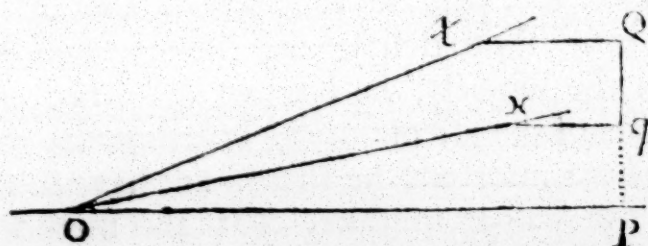
of a very concise demonstration. Suppose a spherical surface IKL , whose center is C , and A to be a point, whence the ray proceeds, and B the eye. AC and BC being joined, and $AC \times EC$ likewise $BC \times CF$ made equal to the square of the semidiameter, and EF drawn and continued; if an equilateral hyperbola FM be described to the diameter EF , whose ordinates shall be parallel to AB , the ray, which passing from A falls on the surface in the point D , where this hyperbola intersects the surface within the angle ACB , shall be reflected to the eye at B . Draw DHG parallel to AB , then by the hyperbola EG is to GD as GD to GF , so that the triangles EGD , DGF will be similar, and the angle DEG equal to the angle FDG . Again, $AC \times CE$ and $BC \times CF$ are equal; therefore AC is to CB as CF to CE , the triangles ACB , FCE are similar and the angle CEF equal to ABC or DHF . Consequently the angles DEG , FDH being also equal, the angles DEC and DFC will be equal. But if CD be drawn, AC is to CD as CD to CE ; therefore the angle ADC is equal to DEC . For the same reason the angle BDC is equal to DFC ; consequently the angles ADC , BDC are equal; and so the ray AD will be reflected to B .



IF the opposite hyperbola be described, it will pass through the point C; and where it cuts the spherical surface within the angle made by AC and BC continued will give the point in the concave surface, whence the ray from A will be reflected to B.

NEITHER ought Dr. Barrow's excellent solution given in his Optical Lectures (a) of the problem in plane refracting surfaces to have been omitted; especially since it admits of an improvement from his Geometrical Lectures (b).

BUT farther in this proposition, what relates to the visible place of the image is inconsistent with Mr. Professor's general doctrine. For ex-



ample, if the object be Qq a part of the line QP , it is here determined, that $Q\chi$ being drawn parallel to OP to meet the reflected or refracted ray, in which the point Q is seen, and $q\kappa$ drawn likewise parallel to OP , till it meet the like ray in κ ; then the object Qq will be seen as extended from χ to κ ; but the general precept was, that the object appears in that place, where it would fill the angle, under which it is seen; whereas here the distance $\chi\kappa$ is greater than Qq .

WE

(a) Lect. 5. § 12.

(b) Lect. 6. § 2.

WE have now gone through the two first books of this treatise, which are intended to comprehend as much of the subject, as is the proper object of mathematicks. In the next book, which promises the mechanical part, to moderate our expectations the author sets out with informing us, that he had himself no skill in what relates to the figuring, grinding and polishing of glasses; and therefore has contented himself with giving us what collection he could gather from others. Thus there is no account of the common methods made use of; but only a particular artifice invented by Huygens, as better suited than the usual ways of working, to the formation of glasses for telescopes of an extraordinary length. But I shall take leave to observe upon this part of the work, that as our author professes to deliver the history and progress of the subjects, he has handled; in the descriptions of the astronomical instruments, which he has here inserted, he should not have subscribed to the vulgar mistake, that the method of graduation used in these instruments was the invention of Petrus Nonius (*a*). For his method of division, which he explains at large in his very curious treatise *de Crepusculis* (*b*) printed at Lisbon in 1542, is widely different. It consists in describing within the same quadrant 45 concentric arches, dividing the outermost into 90 equal parts, the next within into 89, the next into 88, and so on till the innermost was divided into 46 only. By this means, in most observations, the plum line or index must cross one or other of those circles very near a point of division.

(*a*) Art. 861.

(*b*) Part. 2, prop. 3.

vision. Whence by computation the degrees and minutes of the arch might easily be counted (*a*). An example of this very method is to be seen in the first figure of Tycho Brahe's *Astronomiæ Instauratæ Mechanica*, printed in 1597. But as this divides the degrees very unequally, and is also very difficult to execute with exactness, especially when the numbers, into which the arches are to be divided, are incomposite, of which there are no less than nine; though Tycho in his second instrument endeavoured to lessen the latter difficulty by the use of other numbers; yet to that instrument he also added diagonals, which he so much preferred, that at length he left Nonius's way quite off (*b*), and used only diagonals in his mural Arch and the rest of his instruments.

THIS method of diagonals was first published by Thomas Digges Esquire, in an ingenious treatise of his entitled *Alæ seu Scalæ Mathematicæ*, printed at London in 1573, occasioned by the memorable new star, which had

then

(*a*) This artifice Nonius likewise 20 years afterwards describes briefly in his excellent treatise *de Arte atque Ratione Navigandi*, where he would persuade himself, that it was not unknown to Ptolemy, saying, "Ita enim existimo Claudium Ptolemæum fecisse. Nam si maximam solis declinationem idcirco (*ait*) reperisse partium 23. m. 51. se. 20. quia ea proportio inventa fuisset totius circuli ad arcum inter tropicos quam 83 ha-

" bent ad 11. Constat igitur
 " aliquem quadrantem intra
 " ambitum instrumenti descriptum, in ipsos 83 æquales partes distributum fuisse, quarum arcus inter tropicos 44 continebat.
 " Neque enim tanta fuit illius instrumenti quo Ptolemæus utebatur magnitudo, ut in eo prima atque secunda minuta notari possent." Lib. 2. Cap. 6.

(*b*) De Cometa Ann. 1577. pag. 461.

then lately appeared in Cassiopea, on which he made his observations by a forestaff thus divided. But he acknowledges, it was not a discovery of his own, having been long practised in England; and, as he had been informed, was the invention of one Richard Chanseler a most skilful artist (*a*). Tycho, in that part of his *Progymnasmata*, which treats of the same star, takes notice of this passage of Mr. Digges, and affirms, when he studied at Lipsick, he used such an instrument divided after this manner, which was communicated to him from one Homelius, a famous mathematician of that place (*b*). This was about 1565; for in a letter written to Rothman in 1587, he says, he was then 17 years of age (*c*).

BUT

(*a*) —“ libere fateor illam
 “ partiendi radium in pluri-
 “ mas sensibiles partes ratio-
 “ nem a me inventam non
 “ fuisse, sed diu hic in An-
 “ glia a plurimis peritissi-
 “ mis mathematicis usurpa-
 “ tam. Primus tamen qui ea
 “ divisionum ratione usus est,
 “ quemadmodum ex auditu
 “ accepi, quidam fuit nomine
 “ RICHARDUS CHANSLE-
 “ RUS, peritissimus & inge-
 “ niosissimus artifex mathe-
 “ maticus, cujus nomen eo li-
 “ bentius publicare decrevi,
 “ quòd jam e vita discesserit,
 “ neque monumentum sua-
 “ rum virtutum ullum publi-
 “ cum reliquit, præter instru-
 “ menta quædam summa arte
 “ fabricata, & dulcissimam
 “ suæ singularis peritiæ (in
 “ nonnullorum mathemati-
 “ corum adhuc superstitum
 “ animis) memoriam.” Ca-
 pitul. 9.

(*b*) “ Ego certè multo ab-
 “ hinc tempore, videlicet an-
 “ nis plus minus 28, cum
 “ Lipsiæ studiis incumberem,
 “ ejuscemodi partitionem ra-
 “ dii in usu habebam, ex cla-
 “ rissimi mathematici Home-
 “ lii officina, beneficio Bar-
 “ tholomei Schulteti, qui illi
 “ inservierat, mihi communi-
 “ catam. Unde autem Ho-
 “ melius hanc hauserit, aut
 “ an ipsemet eam adinvene-
 “ rit, apud me incertum est.
 “ Sit cujuscunque velit, inge-
 “ niosa certè & apprimè utilis
 “ est distributio, quam & ego
 “ postea arcualibus graduum
 “ subdivisionibus in quadran-
 “ tibus, sextantibus & armil-
 “ lis non inconcinne aut
 “ infrugeterè applicui.” pag.
 671.

(*c*) *Epist. Astronomic.* pag.
 62.

BUT Jacobus Curtius Vice-Chancellor of the Empire, improv'd at different times the division by concentric circles. At first, after describing within the limb of the quadrant a number of such circles, he extended the first to 91 degrees, the second to 92, the third to 93, and so on; and then divided each of these into 90 equal parts. This, when he was the Imperial Minister at Rome, he communicated to Clavius, who published it there An. 1586, in a treatise where he describes an instrument for delineating of dials (*a*); and Clavius himself proposes a variation in this method by describing 39 such concentric circles within the limb of the quadrant, continuing the second of these to 91 degrees, the third to 92, and so on, till the 39th extended to 128 degrees. Each of these circles he divided into 128 equal parts, which is performed by the simple method of bisection (*b*). But a calculation is necessary, or at least a table ready computed, for knowing the degrees and minutes which answer to the divisions of the several circles in both these methods; and Curtius made a second improvement, which required no such computation.

THIS he sent about 1590 both to Clavius and Tycho, which the first published in his book of the Astrolabe, An. 1593 (*c*); and Tycho printed Curtius's letter with the account of this improvement, at the end of the above mentioned treatise, where he gives a description of his instruments. This method consists in setting off upon the first concentric arch within the outermost, the 60th part of such a portion of that arch as answered to 61 degrees, and from that

M

divi-

(*a*) *Fabrica & usus Instrumenti ad Descriptionem Horologior.* Cap. ult. p. 114.

(*b*) *Ibid.* pag. 116.

(*c*) *Lib. 3. Canon. 1. Schol.* pag. 566.

division continuing on through the whole arch the intervals of single degrees. By this means every division in this arch is advanced one minute forwarder than in the first. At the beginning of the next arch he takes off the 60th part of 62 degrees, and from that point continues through the whole arch the intervals answering to single degrees; whereby each division in this arch is advanced two minutes beyond the degrees of the first. And thus he proceeds, till the degrees are divided into the whole number of minutes they contain (a).

BUT

(a) This worthy gentleman Jacobus Curtius was the great patron of Tycho with the Emperor Rodolph the second, when Tycho had been so ill used by his countrymen. He was also a very ingenious person, as appears from these methods of division, as also from a Scale of his invention for readily drawing the hour-lines, and a Quadrant, which by the help of a table of sines and tangents is of excellent use in astronomy and practical geometry. Both these are described by Clavius in the 16th, and last chapters of his above mentioned book of dialling. Curtius likewise made an improvement in trigonometry, which he takes notice of in his letter to Tycho. This was occasioned thus. In 1588 one Nicolas Raymarus Ursus, of whom both Tycho and Rothman often complain as a plagiarist, published at Strasburgh a sort of rapsody, which he called *Fundamentum Astronomicum*, where amongst many things good and bad, at fol. 16 and 17 he gives a scheme

and directions how to resolve several cases in trigonometry by addition and subtraction; this Curtius informs Tycho, he had improved so as to include all cases. He does not there directly set down his method, but only hints how it might be done. Pitiscus at the beginning of the fifth book of his trigonometry in 1600 gives a method to this purpose; but Clavius had several years before in his above mentioned treatise of the *Astrolabe*, Lib. 1. lem. 53. performed it much better. This method may be briefly represented thus. The product arising from the multiplication of any two numbers is equal either to half the sum or to half the difference of the cosines of the sum and of the difference of the arches, to which the numbers proposed shall be the sines, multiplied by the radius; the sum of the cosines is to be taken, when the sum of the arches exceeds a quadrant, and the difference of the cosines, when that sum is less than a qua-

BUT as this last improvement is very op-
 rose, a most excellent compendium of it has been
 since introduced. This was first communicated
 to the world by Peter Vernier a person of dis-
 tinction in the Franche-Comté, in a very small
 tract, entitled *La Construction, l'Usage & les*
Proprietez du Quadrant nouveau de mathema-
tique, &c. printed at Brussels in 1631. In the pre-
 face he claims it as his own invention, and says,
 thereby a quadrant of three inches is rendered
 capable of determining minutes(*a*). In his book he
 shews how to apply it to instruments of different
 dimensions. His contrivance is a moveable
 arch

quadrant. If either or both
 the numbers exceed the ra-
 dius, so as not to be found
 among the sines, such num-
 bers must be divided by
 10, 100, or 1000, &c. till
 they can be found in the
 table, and the produce mul-
 tiplied accordingly. In like
 manner the quotient arising
 by the division of any number
 by another, will be equal to
 half the sum or half the dif-
 ference under the condition
 above mentioned of the cosines
 of the sum and of the diffe-
 rence of two arches, one of
 which has for its sine the di-
 vidend, and the other for its
 co-secant the divisor, that sum
 or difference being divided
 by the radius. But if the di-
 vidend exceed the radius, it
 must be divided as before;
 and if the divisor be less than
 the radius, it must be multi-
 plied in like manner, till it
 can be found among the se-

cants. This directly follows
 from these two propositions;
 The first, that the rectangle
 under the sines of two arches
 is equal to the rectangle un-
 der the radius and half the
 sum or half the difference of
 the cosines of the sum and of
 the difference of the arches.
 The other is, that the sine
 and co-secant of the same arch
 are reciprocals.

(*a*) “ — d'un instrument
 “ du tout admirable de mon
 “ invention, & qui n'a ja-
 “ mais esté veu, &c. — en
 “ un quart de cercle de trois
 “ poulces d'estendue, on de-
 “ melle les minutes entieres
 “ du cercle quoy qu'il ne soit
 “ divisé, qu'à l'ordinaire en
 “ nonante degrées, à l'ayde
 “ toutefois d'une petite por-
 “ tion de cercle mobile con-
 “ tenant seulement quinze
 “ parties, qui sont toutes les
 “ subdivisions necessaires au
 “ dict instrument. —

arch divided into equal parts, one less in number than the divisions of the portion of the limb corresponding to it (*a*).

IN 1634 Joan. Baptista Morinus, one of the Royal Professors of mathematicks at Paris, printed a book entitled *Longitudinum Cœlestium atque Terrestrium Scientia*. The way there deliver'd for finding the longitude had been publicly examined by commissioners appointed by Cardinal Richelieu. The author in graduating the instruments for making observations to sufficient exactness commends two methods. The first, he says, was the invention of Joannes Ferrerius, a most industrious and accurate artist (*b*). This is performed by circular diagonals, which continued shall pass through the center of the qua-

(*a*) In his first instrument after having supposed a quadrant to be divided into half degrees, he adds, “ La seconde partie de l'instrument est une planche construite en la forme & figure d'un secteur de cercle, la circonférence de laquelle comprendra justement un angle de trente & un demy degrés, non toutefois formés ny décrits, mais bien divisé en trente parties esgales seulement :—” p. 9.

(*b*) “ Prima. Est accurata & geometrica divisio cuiuslibet gradus in 60 minuta, quæ super Quadrante 2 pedum, imò unius pedis semidiametri commodè applicari potest; quam adinvenit D. Joannes Ferrerius instru-

“ mentorum mathematicorum solertissimus & accuratissimus fabricator, qui præsens aderat; ipsis notissimus Commissariis, quibus inventionem suam pridem detexerat.” *Long. Scient. Par. 1. p. 17.* This Ferrerius must have been then very old, if he was the same that is praised for finding out a new method of drawing the hour-lines upon dials by Clavius, in the preface to his above mentioned book printed in 1586, in these words; “ Inventor primus hujus rationis, quæ præclarissima est, Hispanus quidam dicitur, nomine Joannes Ferrerius, homo in primis acutus, & in rebus inveniendis admodum sagax.

quadrant, and which he demonstrates to answer to geometrical rigour (*a*). But to save the trouble of drawing particular diagonals for every degree he advises one only to be affixed to the moveable index, which being divided into 60 equal parts, by its intersection with the streight lines proceeding from the center and distinguishing the degrees on the limb of the instrument, will give the overplus minutes (*b*). The other method is Vernier's (*c*), which Morinus particularly explains, and shews, if the radius of the quadrant be half a foot, one foot, two feet, or three feet, by a small arch divided into 30 equal parts answering to 31 of the divisions of the limb, along which it moves either as a sector, or as joined to the arm that carries the sights, angles may be measured true to one minute, 30 seconds, 15 seconds, or 6 seconds respectively. This he justly prefers to the other method (*d*), and describes it again in the continuation of his book, printed in 1636 (*e*), and again in a further continuation printed in 1639, where he recommends an azimuth quadrant of 5 feet radius, which by this artifice would measure angles to two seconds (*f*); and lastly, in his answer to Longo-

(*a*) Long. Scient. Part 2. pag. 51, &c.

(*b*) "Hæc enim particula
"semper adhærens alhidadæ
" & secta à lineis quæ dis-
"tinguunt gradus instrumen-
"ti, ostendet in sectione mi-
"nuta gradibus addenda."
Ibid. p. 52, 53.

(*c*) "Secunda est accurata
"etiam divisio 31 graduum
"five partium Quadrantis in

"30 partes æquales; inventa
"& in lucem edita à Nobili
"viro Petro Vernerio in co-
"mitatu Burgundiæ castelli
"Dornansii Capitaneo; quæ
"cum Quadrante 2 pedum
"quarta pars unius minuti
"sensibiliter mensuratur."

Ibid. Par. 1. pag. 18.

(*d*) Ibid. Par. 2. pag. 53.

(*e*) Par. 6. pag. 187.

(*f*) Par. 9. Cap. 8.

Longomontanus, printed in 1641, he mentions both these methods (*a*).

NOTWITHSTANDING all this, in 1643 one Benedictus Hedræus a Swede, published at Leyden a small treatise, entitled *Nova & Accurata Astrolabii geometrici Structura*, &c. where in the preface he takes occasion to describe circular diagonals without mentioning Ferrerius, and assumes to himself Vernier's method. This in his book he applies to a Theodolite and Protractor, by dividing such a part of a moveable circle as answers to 61 degrees of the limb of the instrument, into 60 equal parts (*b*); he shews also how this moveable circle may be otherwise divided (*c*), and likewise describes a large astronomical quadrant, which by a small moveable arch corresponding to 5 degrees 5 minutes of the limb of the instrument, each degree of which limb being divided into 12 equal parts, would measure angles to 5 seconds (*d*), or even half that.

THIS method is also succinctly explained by Tacquet in his *Opera Mathematica* (*e*) printed in 1669, where he would ascribe the original of it to a proposition of Guido Ubaldi in his book of *Astronomical Problems* (*f*) published after the author's death in 1609. But this was de-

(*a*) *Coronis Astronomiæ*, p. 14.

(*b*) "Limbo sic diviso, numerentur ab una linearum, quas vocant fiduciæ, ut in delineatione AB, in quam partem placuerit, 61 gr. ab A usque in C, & spatium illud in circulo mobili, quod hisce

"61 gr. respondet, dividatur in 60 partes æquales." Par. 1. Memb. 1. cap. 2.

(*c*) Ibid. cap. 6.

(*d*) Par. ult. Memb. 1. cap. 2 & 3.

(*e*) *Geom. Pract. lib. 1. cap. 4. pag. 23.*

(*f*) *Lib. 1. Prob. 1.*

delivered very distinctly by Clavius in 1586 (a), as the invention of Fabritio Mordente, the Emperor Rodulph's mathematician, who discovered it long before, as appears from a treatise of his brother printed at Antwerp in 1584, entitled *Il Compasso del Signor Fabritio Mordente: con altri istromenti mathematici ritrovati da Gasparo suo fratello*. Though this can at most be considered only as a very faint rudiment of these methods, being no more than a proposal to find the number of minutes, seconds, &c. of any arch less than a degree, by multiplying this arch 60 times, whereby the number of degrees, it measures, will express the number of minutes in the original arch; and if the multiple exceed an exact number of degrees, the excess multiplied in like manner will exhibit the seconds, whereby the arch given exceeds an exact number of minutes.

IN 1673 the famous Hevelius published the former part of his *Machina Cœlestis*, where he adopts this method with due applause; being the first that reduced it to practice in large astronomical instruments. He describes it from Hedræus, and appears not to have known, that it had been mentioned by other writers.

IN 1674, one Gerard à Gutschoven, Professor of the Mathematicks and Anatomy in the University of Louvain in a very small piece entitled *Ufus Quadrantis Geometrici* printed at Brussels gives this method without taking notice of any preceding author. He applies it in the form of a sector (b) to a small quadrant, whereby

(a) *Fabrica & Us. Instrum.*
&c. cap. ult. pag. 120.

(b) "*Cursor est sector circuli mobilis, cujus partes*

"*sunt arcus minutorum, arcus*

"*nempe in 30 partes æquales*

"*divisus, ipsos Quadrantis*

"*gradus lambens.*" in præf.

whereby minutes may be measured (*a*). This quadrant, which he so much boasts of, as not the production of his closet, but as brought into use in the fields and woods, has the rectilinear sides of the quadrat projected into two circular arches to make room for the sinical division in the middle (*b*), which had been described by others (*c*).

LASTLY, it is this method of Vernier, that Mr. Professor Smith has also described, which is indeed preferable to any form of diagonals, though Dr. Hook in opposition to Hevelius labours to prove the contrary (*d*). He has indeed removed the objection against the absolute exactness of rectilinear diagonals, when divided by equidistant concentric arches, by shewing how to divide them more truly by the help of a table of tangents (*e*). But as his arguments for their preference are very weak; so he in
vain

(*a*) "Quadrans totus divisus est in 90 gradus; Cursor autem in 30 partes æquales qui comprehendit accurate 31 gradus quadrantis. Quisque Cursoris gradus singulos Quadrantis gradus excedit unâ trigessimâ gradus, id est, duobus minutis primis, unde fit ut sequenti methodo non gradus tantum, sed etiam singula minuta prima à Regula fiduciæ abscissa numerare valeamus." — Prob. 1.

(*b*) "Usui & praxi maximè appropriatum [instrumentum] mihi excogitavi non in Musæo vel Cathedra, sed in ipsis agris & silvis,

"in ipsa manuali operatione." "In præf. And afterwards "Habes Scalam altimetram ad arcum reductam variis usibus destinatam: — Tandem planum opplevimus lineis cancellatis, magnum in triangulis solvendis usum habentibus; & hæc etiam de penu meo deprompta sunt."

(*c*) See Santbech Problem. Astron. & Geom. printed in 1561. Sect. 1. Hopton's Bacculum Geodæticum in 1610. B. 6. Chap. 11. and Speculum Topographicum in 1611. chap. 4.

(*d*) See his Animadversiones on Hevelius's Mach. Cœlest.

(*e*) Ibid. pag. 22. and 23.

vain would alledge the authority of Tycho on his side: for it does not appear from Tycho's works, that this compendium was at all known to him. The citations (*a*) thence given by the Doctor, refer only to that little improvement upon Nonius mentioned above.

THE truth is, neither of these methods are of any use unless executed with exactness; and one great excellence of that of Vernier is its readily discovering the least error committed in the divisions. For which reason none but an able artist dare undertake it. But it is now performed very accurately in various instruments by our most skilful workmen, and its superior usefulness is sufficiently established by its having a place in the mural arch of our Royal Astronomer; whom the same ardor still supports, which above 60 years ago carried him to St. Helena to settle the southern constellations; so that we may expect from his great abilities, and unwearied diligence, a series of observations upon the Moon's motion, that will far exceed whatever has been produced since the very beginning of astronomy.

BUT to return to our author, whose fourth and last book promises a compleat history of the discoveries, that had been made in the heavens by the telescope, selected from a great variety of books, memoirs and observations of the best astronomers; which books are these, Galileo's Nuncius Sydereus the first on the subject, the Systema Saturnium of Huygens, Du Hamel's History of the Royal Academy of Sciences at Paris, the Memoirs of that Academy, our Philosophical

(*a*) Ibid. pag. 3, & 4.

velocity of the earth in B is to the velocity in C reciprocally as DB to DE; that is, directly as AD to DG, since the rectangles under ADB and EDG are equal. Now CH being drawn from the earth in C toward the true place of any star, take CI to AD as the velocity of light to the velocity of the earth in B. Through I draw KIL in a situation perpendicular to AB, and making KI equal to AD and IL equal to DB, upon the diameter KL describe the circle KMLN parallel to the plane ACBF. Lastly, make the angle under KIO equal to that under ADG, which is nearly the arithmetical mean between the mean and true distance of the earth from the aphe-
 lion, and draw CO. Now KL being in a situation perpendicular to AB and the angles under KIO, ADG equal, OI will be equal to DG, and be situated perpendicularly to GE, and therefore is parallel to CE or to the direction of the earth's motion in C; and is to KI as the velocity of the earth in C to its velocity in B. Consequently since KI or AD is to CI as the velocity of the earth in B to the velocity of light, by equality OI is to IC as the velocity of the earth's motion in C to the velocity of light. Therefore the star will be seen from the earth at C in the direction CO. Hence it follows, that every fix'd star is viewed from the earth as moving in a circle parallel to the plane of the ecliptic, the diameter of that circle which is parallel to the lesser axis of the earth's orbit being divided by the true place of the star in the same proportion, as the greater axis of the earth's orbit is divided by the sun.

REMARKS

O N

Dr. *JURIN*'s ESSAY

U P O N

Distinct and Indistinct VISION.

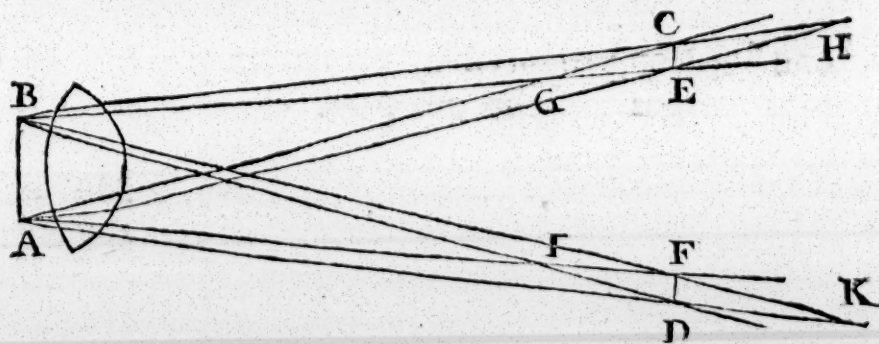
TO these animadversions upon Mr. Professor Smith's compleat System of Opticks I sh all add some observations upon the Essay subjoined by his friend and fellow-writer Dr. Jurin. This discourse consists of four general heads. The first relates to the difference in the appearance of an object when seen distinctly, and when confusedly ; the second to that faculty in the eye, whereby it accommodates itself to the different distances of objects ; the third to a particular phænomenon in indistinct vision, which is the multiplication of the object ; and the fourth is taken up in considering the dazzling, to which the eye is subject by the immediate contrast of light and darkness.

A S it appears, that this gentleman learnt the principles of opticks from his friend's book ; so in his first head he has expatiated on the simple case of a uniform white object upon a black ground seen indistinctly more in detail, than one can suppose, a writer would have done, to whom the subject was not new : And after all he has treated

REMARKS on *Dr. JURIN.* 93

ted of it but imperfectly, proceeding on a supposition, that the light is uniformly spread thro' his circles of diffipation ; whereas he confesses at art.220. that the density of the light is different in different parts of these circles. Nay, he had so ill fixt the subject in his thoughts, that when he was put to such hard shifts how to account, that the astronomers without the assistance of telescopic sights should observe the diameter of the moon nearer to the truth, than he could have expected, he had certainly forgot, that the magnitude of his circle of diffipation depends upon the breadth of the pupil ; for as those observations were made either through a small hole, or slit much narrower than the pupil, the dilatation of the moon's image from its indistinctness must have been proportionably lessened.

WHEN he comes to consider at art. 90. an object but a little more complex, a black circular line seen upon a white ground, or two black parallel lines viewed in like manner, he has much mistook. It is a fact, that such a circle at one distance from the eye will appear to have a black spot in the middle, and the middle space between two parallel lines near together in the like situation will become dark ; but this is owing to a very different cause from what he has assigned : For if *AB* be the diameter of the pupil, and *CE*,



FD the distinct images of the transverse sections
of

of the circumference of the circle, or of the parallels; and from the extremities of the pupil to the extremities of these images the rays AC, AE, AF, AD, and BC, BE, BF, BD be drawn, the spaces GCHE and IFKD will be deprived of light; but all without these spaces will be so much illuminated, that the axis of the eye can nowhere be deprived of light sufficiently to produce so distinguishable a degree of darkness. Neither is the appearance truly described by our author, for the middle darkness in the parallel lines, and the central spot in the circle do not extend into a simple penumbra gradually vanishing, but that shade is composed of a number of different images, and appears uniform, only when those images become so numerous, as not to be easily distinguished. And the true cause of this appearance is the multiplication of the image found in objects seen indistinctly; which I apprehend to arise from some corrugation, or inequality of surface, to which that part in the eye, which is changed for the different distances of objects, is subject in its extreme tension either way, whereby the image of an object look'd at out of the limits of distinct vision is multiplied. From this multiplicity of images two parallels, as each is multiplied, at first are seen as four lines, or even more, but at length the two innermost unite into one; and in the circle by a more multifarious appearance a spot in the center is in one situation produced by the union of the different images in some one part.

MORE-

MOREOVER the defence of Hevelius against the objections of Dr. Hook, which this gentleman is here pleased to bring in, serves only to shew, he did not understand the method of observing practised by that great astronomer. It is very well known, that Dr. Hook passed too severe a censure upon Hevelius's instruments, and his method of observation. But this mere imagination of an extraordinary length of sight, which Hevelius might acquire from age and long exercise is a very trifling defence. But the truth is, his method of observation required no such length of sight. This might be justly objected to the method of observing practised before Tycho Brahe; but that noble restorer of astronomy invented a form of sights continued in use by Hevelius not chargeable with this inconvenience. The sight next the eye is a plane furnished with two parallel chinks, both of which, not one only, as this gentleman (*a*) has represented, are made use of in every observation. The other sight is either a plane likewise equal in breadth to the interval between the chinks, or a cylinder of that thickness. Now when these sights were pointed towards any star, a little before they came into the exact direction of the star, the light of the star would shine through one of the chinks, but be intercepted from the other by the interposition of the remoter sight, and when the sights had passed the direction of the star, the chink before obscured would now receive the light of the star, and the other chink be covered from it. But in the intermediate situation, which is the very direction of the star, the light of the star would pass through both chinks. Therefore it was the business

(*a*) Essay upon distinct and indistinct Vision, art. 183.

finess of the observer to bring the sights into such a situation, wherein by looking through each of the chinks, one immediately after the other, his eye would receive the light of the star through both. This method of observation therefore is little influenced by the larger or shorter sight of the observer, but its accuracy depends upon the proportion of the breadth of the chinks to the distance between the sights: and those chinks were so framed, that by a screw or some other like contrivance they might be equally dilated, or contracted together. In this manner of observing Dr. Hook's assertion is evidently false, that a lesser angle cannot be distinguished by a long radius than by a short one (*a*).

I N the second part of this essay, after informing us, that he had deceived himself by certain trials to favour the singular opinion of Monf. de la Hire, that the eye was not possessed of the faculty, he now admits of, he tells us, that Dr. Porterfield in his dissertation lately published by experiments better contrived has caused him to change his mind, and subscribe to the common opinion.

I N relation to the new scheme, he advances, concerning this change in the eye; before it is any further considered, I think, we must ask the anatomists, whether a muscle strong enough to keep the eye tense can be invisible.

I N his third head this gentleman has had the hardiness to advance beyond the instructions of his master; and has adventured to expatiate upon the discoveries of Sir Isaac Newton, concerning
ing

(*a*) Animadversions on Machina Cœlestis, pag. 7, & 8.

ing the fits of easy reflection and transmission of light, where Mr. Professor has chose to be very concise. And as our compleat system of opticks is upon this argument so very deficient, that these alternate fits of reflection and transmission of light are not so much as mentioned by name, where he has undertaken to exhibit Sir Isaac Newton's discoveries in optics (*a*) ; before I come to consider this writer's speculations upon this point, I shall briefly give some account of this matter.

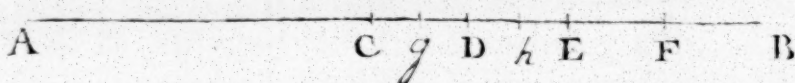
IT is well known, that when light falls upon any the most transparent substance, a part only enters the substance, some part being reflected at its superficies. Again, when the light, which passes into the substance, is arrived at its farther superficies, the whole is not transmitted through that superficies, but there also a part is reflected back. When any colourless transparent substance is exposed to the open light of the day, what is reflected from its first superficies is always white, and so is the light returned from its farther surface, unless the substance be very thin; for in that case the light is coloured; and the colour differs according to the particular thickness of the substance. If such a thin transparent plate be unequally thick, the light reflected from its farther surface will be variegated with different colours. Such is the appearance seen in a bubble blown up with water rendered tenacious by the dissolution of a little soap. When this bubble is placed under a glass to protect it from being disturbed by the agitation of the air, the water, which composes it, will gradually and regularly descend from the upper to the lower
O part,

(*a*) Book 1. chap. 6, 7, 8.

part, rendering the bubble continually thinner and thinner till it breaks ; the upper parts also by this means will be always thinner than the lower. This bubble, when grown very thin, will be overspread with a great variety of colours, which Sir Isaac Newton has very particularly described. Likewise, if the object-glass of a very long telescope be laid upon a glass, that is flat, so as to touch it in one point, the small convexity of the telescope glass will leave between the two a thin plate of air of a considerable breadth perforated in the middle, where the glasses touch, and from thence gradually thickening. Here, where the glasses touch, and for some little distance round, the light is so freely transmitted, that no reflection is seen, but this dark spot will be encompassed round with a great variety of colours of the same kind with those seen upon the water-bubble. When the plate of air begins to be a little thick, these colours become of a very compound kind, and soon after end in perfect whiteness. In like manner the light reflected from the bubble of water at first, while its coat is thick, is uniformly white ; and on the contrary, when it becomes extremely thin, a dark spot appears on the top by the reflective power being in that place nearly quite lost.

THE cause of these phænomena Sir Isaac Newton found to be, as follows. The rays of light in their motion through the air, or any other transparent medium are not alike disposed in every part of their passage in regard to reflection and refraction ; in so much that in some places, if they meet with another transparent substance, they shall enter it freely, suffering only

a refraction at the surface upon its transmission through into the transparent substance, but in other places the same ray shall be disposed to be reflected back from any such substance in its way. These alternate dispositions are found to return at equal intervals after this manner. Suppose AB to represent the path of a ray of light.



Then if at the point C the ray be disposed to enter freely any transparent substance interpos'd in its way, but at D be disposed to be reflected, from the surface of such a substance; if DE be taken equal to CD, at E it shall again be in a disposition freely to be transmitted through the superficies of such a surface, and if EF be taken of the same length with the former intervals, the ray at F shall be again in a disposition to be reflected. And thus will these two dispositions change and return at equal intervals throughout the whole progress of the ray within the same medium. But these alternate dispositions are not to be understood, as confined to these precise points; but if these dispositions are supposed to operate strongest at these points, they must be considered, as decreasing gradually from them both ways. Sir Isaac Newton divides these intervals into two equal parts, as here CD is divided at *g*, and DE at *h*; and considers the whole fit of reflection to be extended between *g* and *h*, but strongest at D, and from thence gradually weakening toward *g* and *h*, yet with this caution that it should not be conceived as precisely limited at these points *g* and *h*, but to decay indefinitely.

BUT farther, as the rays of light differ one from another in colour, and refrangibility, in so much that there are rays of all degrees of refrangibility indefinitely from the least to the greatest; so the lengths of these fits of easy reflection and transmission are as much varied, and every degree of refrangibility is accompanied with a peculiar length of these fits, the least refrangible rays having their fits the longest, and the most refrangible rays the shortest within the same medium.

AGAIN in different mediums, and according to the different angle, under which the ray enters any medium, these fits are in the same rays different.

THE inequality in the length of these fits belonging to rays of different refrangibility and colour, is very great within the same medium, the intervals between the middle points of two nearest fits either of reflection or refraction in the most refrangible rays being to that length in the least refrangible as 63 to 100; whence it follows, that when the medium through which the light passes, is so thin a plate, that its whole depth contains but a small number of these fits in any one colour, the light of some colours may at its farther surface be in a fit of easy reflection, and the light of other colours in a fit of easy transmission, in which cases the light reflected will be coloured. Sir Isaac Newton exhibits for an instance of this the thickness of a plate, wherein the green light shall at the farther surface be in its third fit of easy reflection; there the yellow, and great part of the blue will in some degree

gree be in a fit of easy reflection also ; but no other part of the light : therefore the reflected light will be pure green compounded only with some yellow and blue.

HOWEVER to produce this coloured appearance the plate must be very thin ; for these intervals in every one of the colours are very small ; in the least refrangible rays, when the light falls perpendicularly upon a plate of air, the interval between the middle of two successive fits of easy reflection, that is, *DF* in the preceding figure, is not the 78000th part of an inch, and in the most refrangible rays scarce the 125000th part. In water these fits are about $\frac{3}{4}$ only of this length, and in glass not quite $\frac{2}{3}$. If the plate of air were but the 800th part of an inch, whereby according to computation from Sir Isaac Newton's numbers, the least refrangible rays would arrive at the farther surface in the 99th fit of reflection, a ray at the confine of the red and orange will have 107 fits of reflection within the plate, a ray at the confine of orange and yellow will have 111 fits of reflection, a ray at the confine of yellow and green 119 of those fits, a ray at the confine of green and blue 129 fits, a ray at the confine of blue and the indico 139 fits, a ray at the confine of the indico and violet 145 fits, and lastly the most refrangible rays 156 of these fits. Hence many species of the rays of each denomination of colour will be found in fits of easy reflection at the farther surface of such a plate ; all which together can compose nothing short of perfect whiteness in the reflected light, and consequently the light transmitted will be equally free from any tinge of colour. Nay so soon does the light lose all diversity of colour,
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that Sir Isaac Newton found it in his observations to become white, as soon as it had more than 8 or 9 fits in passing through a thin plate (*a*).

ANOTHER circumstance is here still to be considered. While the transparent plates in Sir Isaac Newton's experiments were so thin as to exhibit rings of colours, the rings, where the plate was thinnest, reflected the greater quantity of light. The reason of this is evident from the method he has laid down for finding the colour reflected in each case, whereby it appears, that in the most luminous part of the rings, where the plate was thinnest, a greater proportion of the whole light was in a fit of reflection at the farther surface of the plate. But when any plate comes to be so thick, that the light of each denomination of colour has many parts reflected, and many parts transmitted, by reason that the whole contains many species, which shall have different numbers of fits of easy reflection or easy transmission; then the light reflected will not only be white, but the number of the species of light in fits of reflection will be so equal to the number of those in fits of transmission, that the proportion between the quantity of reflected and transmitted light will not be varied by any increase of the thickness of the plate.

FOR instance in the plate of air before mentioned the 800th part of an inch in depth the extreme red light in the middle of a fit of easy transmission at the first surface, would arrive at the farther surface in the middle of the 99th fit of easy reflection, and the extreme violet in the middle of a fit of easy transmission at the first surface.

(*a*) Optics B. 2. part 1. observ. 12.

surface, would arrive at the farther surface rather beyond the verge of the 156th fit of easy transmission ; infomuch that, as there would be at least 57 different species of rays, which would arrive at the farther surface after a different number of fits of reflection, so there could not be reckoned above one less in fits of transmission. Were the plate the tenth part of an inch in thickness, the number of species arriving at the farther surface after different fits of reflection would be no less than 4629, and the number of species in fits of transmission cannot at most differ from this above one. Hence it come to pass, that in all thick plates, and consequently in all the common appearances, the light reflected from every point of either surface of colourless transparent substances, whatever be the figure of their superficies, is not only always white, but also the same in quantity of light, the same proportion of the whole light arriving at each point of such surfaces in fits of easy reflection.

HENCE appears how absolutely this gentleman has erred in this whole matter, when, in order to account for the double, or more compounded appearance of objects seen indistinctly he imagined, that by reason of these alternate fits of easy reflection and transmission whenever a pencil of rays, from a distant object falls upon the cornea ; that because some parcels of its rays will be transmitted into the eye, while other parcels are reflected back into the air, therefore, if the light were received upon a plane placed before the crystalline, it would consist of a middle circle surrounded with rings dark and luminous alternately.

NEITHER can any diversity be made, as this gentleman farther supposes, in the transmitted light at either surface of the crystalline; for the brightest part of the light in passing between the cornea and crystalline, which is the least of the two intervals, and that wherein the fits are longest, will have more than 12000 fits of reflection, or refraction.

BUT as little as this gentleman appears to have understood the subject, he had far other thoughts; for he has not contented himself with applying this curious doctrine, as he terms it, to vision, but even takes upon him to correct and improve upon Sir Isaac Newton by making the fits of reflection of different extent from the fits of transmission in a manner contrary to what Sir Isaac Newton has represented. From what we said above, it appears, that in common half the light incident upon any surface is in a fit of transmission, and the other half in a fit of reflection. Hence arises this obvious question, Why is not half the light reflected at all surfaces? But in regard to this it must be considered, that Sir Isaac Newton has pursued his speculations upon this subject no farther, than what he could determine by indubitable experiments and observations, and he has found, that at all surfaces, where the light is acted upon by bodies, as a part is transmitted into pellucid substances, so a part is reflected; and he likewise finds, that as the transmitted light is diverted from its straight course by a refractive power, where that power is strongest, the greatest quantity of light is reflected. From this and other considerations he concludes, that it is one and the same power, by which

which the light is refracted and reflected; and that the light, which is reflected, is only such, as is in a fit of easy reflection, when it arrives at the surface; and as these fits in the passage of the ray to the transparent substance are alternately changed for fits of transmission, and return at certain intervals; so each fit likewise continues for a certain space, and that the intervals of these returns, and the space, through which each continues, depends altogether upon the medium, through which the light passes, and not upon that, whereon the light falls. But as Sir Isaac Newton had not discovered, wherein this power in bodies of refracting and reflecting light expressly consists, so he is silent in relation to the cause, why more light is reflected from some bodies than from others.

Now this gentleman imagines this defect in Sir Isaac Newton may be supplied by correcting him in regard to the extent of each fit of reflection, and supposing that to be varied according to the medium, upon which the light falls; for instance, when the light passing through air upon glass, or through glass upon the air, the extent of the fit of easy reflection should not be what Sir Isaac Newton supposes, half the interval between the middle of the nearest fits of reflection, but only $\frac{11}{100}$ of that interval, and at surfaces between air and water $\frac{6}{100}$ of that interval. But this supposition is absolutely inconsistent with the very experiments, from which Sir Isaac Newton deduces his whole doctrine. If the extent of these fits varied thus, not only the quantity of light reflected from a transparent surface would
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be varied by changing the contiguous medium, but also the colour, which, Sir Isaac Newton says, he could never observe. The colours, which arose successively upon the bubble of water, Sir Isaac Newton has described very distinctly with all the concomitant circumstances; but were it examined according to the new regulation of our author, what colours ought to have appeared, we shall find, that from the yellow, which Sir Isaac Newton ranges in the 5th order of colours, to the total blackness, each colour ought to emerge almost uncompounded, being, where the least simple, scarcely mix'd with the contiguous colours of the same order, and never with those of any other. In particular the purple in the 5th order, which, Sir Isaac Newton says, was much inclined to red, must have been a pure violet without any mixture, and a dark interval have been seen between that and the red of the next order; even in the 7th order, where they are most compounded, no thickness of the bubble can be assigned, at which more than three colours will be reflected; insomuch that in no part of this 7th order of colours any whiteness could appear: nay were the extent of the fit of easy reflection any thing less than, what Sir Isaac Newton supposes, a sensible interval void of reflection must have appeared between this and the order preceeding. This shews, that the proportion assigned by Sir Isaac Newton between the extent of each fit of easy reflection and the interval of the fits was the result of mature deliberation, and necessary towards producing the appearances.

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HENCE it appears, that whether more or less light be reflected from a body, the extent of the fits of reflection in the incident rays is the same, and the difference in the quantity of reflected light is owing to the greater or less portion of that part of the light, which arrives at the body in a fit of easy reflection, being thrown back by the reflective power of the body. The cause this gentleman has been pleased to assign for this, is so absolutely inconsistent with the essential principles of Sir Isaac Newton's doctrine, that the thought could never have been entertained by one of the least degree of skill in the subject. But this gentleman seems so great a stranger to the matter, he has writ upon, that he does not appear in the least to have known, that rays of different colours have their fits of reflection and transmission of different lengths. For from this it follows, that, when a thin transparent plate, whether the bubble of water in Sir Isaac Newton's experiments, or the plate of air between the flat and convex glass, is exposed to the open light of the day, scarce any part of it is exempted from reflecting light, except the central spot only, but the rest appears spread over with different colours contiguous to one another. But had this gentleman been at all apprized of this, he should at least have suspected, that if the phenomenon, he is here endeavouring to account for, depended upon the cause, he assigns for it, instead of a multitude of separate images a broad one should have presented it self variegated with colours.

AFTER it has been thus made appear, how totally unskilled this gentleman is, in this doctrine relating to the fits or dispositions of easy reflection

and of easy transmission accompanying the rays of light, I know not, whether it may be thought worth while to take notice, how little this gentleman's erroneous opinions concerning it will serve his purpose. By computing on supposition that rays of all colours have the same intervals between their fits, and the extent of each fit what this gentleman assigns, the magnitude and number of the rings, that are formed at the cornea, when the rays are incident in such a manner as to make the rings the largest, will be above 670, if the aperture of the pupil be supposed $\frac{1}{10}$ of an inch, and if the magnitude of the first or largest dark ring be desired, it will be found, that, if the rays falling on the middle of the cornea be in a fit of easy transmission, the breadth of the first dark or reflected ring will be about the $\frac{1}{500}$ of the whole pupil; but if the rays falling on the middle of the cornea, be in a fit of easy reflection, then the breadth of the first dark ring will be but about the $\frac{1}{665}$ part of the whole pupil; if we conceive now the vision to be so indistinct, that a luminous point appears under an angle of 20 minutes (a degree of indistinctness five times greater than that, under which he supposes these rings to be perceptible) the breadth of the first or broadest dark ring cannot under all these favourable circumstances subtend a greater angle at the eye than $2\frac{2}{3}$ seconds; and consequently none of these rings could even on his own representation of this doctrine be ever sensible. It is evident then, that, when our author to account for the double appearance of objects seen by indistinct vision supposes the visibility of these rings, it was

was not only necessary, that he should utterly misapprehend Sir Isaac Newton's doctrine; but that he should also be ignorant of the most obvious consequences of those absurd conceptions, he had formed to himself on this subject.

IN the last part of this essay our author has discoursed very diffusely upon the dazzling, to which the eye is subject from the immediate contrast of light and darkness, and tells us, he should not have been so particular, had he not suspected, that Sir Isaac Newton was once led into a mistake by an appearance similar to those, he here treats of; at least, says this gentleman, I do not find, he has any where accounted for the extraordinary strength of the ring of light next the central black spot spoken of in Observation 23, Part 1. of his second book of Opticks; nor do I see any way of accounting for it, but from some such consideration.

HERE it is supposed, first, that Sir Isaac Newton in attributing any superior degree of brightness to this ring was deceived by the neighbourhood of the central black spot; secondly, that Sir Isaac Newton has no where accounted for this extraordinary brightness; and lastly, that no account can be given of it, but what our author here assigns.

If this were indeed a mistake of Sir Isaac Newton, it must be owned a very signal one; since he so little suspected this appearance to be owing to any such incidental cause, that when he is ranging the colours of bodies under the several rings produced in these experiments, he places the light of white metals under the class
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of this ring on account of their superior brightness. But it is further evident, that he was not imposed upon by any such deception, from the method he took to satisfy himself of the extraordinary brightness of the light of this ring, by comparing two bubbles of water blown at distant times; in the first of which the whiteness appeared, which succeeded all the colours, and in the other the whiteness, which preceded them all (*a*), and before (*b*) he tells us, this whiteness, which succeeded the colours, would often spread and dilate it self over the greater part of the bubble, before that even the succeeding blue emerged on the top; nay, would decay gradually, and quite vanish, before the black spot appeared.

AGAIN, though Sir Isaac Newton has not separately discussed the cause of this particular appearance, yet it is so unnecessary to have recourse to this gentleman's chimerical imagination concerning it, that it is a most obvious consequence from the method, Sir Isaac Newton gives for investigating the several colours exhibited by thin plates. For it thence appears, that at this thickness of the plate no part of the light, which enters it, arrives at its farther surface in a fit of easy transmission, which never happens in any greater thickness.

THUS then has this learned gentleman unhappily attempted the application and improvement of a doctrine, of which, it appears, he was not qualified to describe even the very rudiments.

FOR

(*a*) Opticks, pag. 196.

(*b*) Ibid. pag. 190.

FOR the first principle laid down by Sir Isaac Newton in the second part of his second book of Opticks is the inequality of the length of the fits of transmission and reflection in rays of different colours, and it is only from the combinations of these different lengths in plates of a different thickness, that all the appearances described in the first part are deduced.

BUT the description and application of this doctrine, as given us by our author (*a*), do necessarily suppose the lengths of these fits to be the same in all rays however different in colour.

AGAIN, Sir Isaac Newton in the same ray makes the fit of reflection equal in length to the fit of transmission, the experiments in the first part necessarily requiring it.

ON the contrary Dr. Jurin tells us, that in water the fit of reflection is in length but $\frac{6}{94}$ of the fit of transmission, an assertion that absolutely destroys the whole theory established by Sir Isaac Newton, setting it at variance with almost every appearance, it is intended to solve.

LASTLY, The immediate consequence of Sir Isaac Newton's theory, as described in the second part of the second book of Opticks is, that the light of the first ring surrounding the central spot should be much stronger than that of the succeeding rings, all the rays being there in a fit of reflection.

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(*a*) Art. 209, 210, 211, &c.

112 REMARKS *on Dr. JURIN.*

YET so little was our author versed in this doctrine, that he conceives the remarkable strength in the light of this ring to be only an illusion, suspects Sir Isaac Newton to have been mistaken in his description of it, and tells us, that he does not see, that its reality can be any way accounted for.

FINIS.



